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IN
HISTORICAL CHEMISTRY

BY
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PREFACE

THIS book consists mainly of lectures and addresses given at various times, and to audiences of very different type, during the last forty years. These essays in historical chemistry are now put together with the object of showing how the labours of some of the greatest masters of chemical science have contributed to its development. The book has no pretensions to be considered a history of chemistry, even of the time to which its narratives relate. Many honoured names—as Black, Dalton, Berzelius, Liebig, Hofmann—that ought, in all fitness, to find a fuller notice in such a series of biographical sketches, are only incidentally mentioned. The only excuse I can advance is, that it has not as yet been my good fortune to be in a position to offer an account of their labours.

The greater number of the sketches in the present volume have already been seen in print; but in arranging them for republication I have not hesitated to make such alterations and corrections as seemed necessary or desirable in view of their appearance in a connected series. Certain of the lectures, when delivered, were illustrated by experiments of which mention was made in the accounts originally published. It seemed useless to retain these references, and they have consequently

been omitted. The lectures, too, for an obvious reason, have been arranged in historical sequence, and not in the order in which they were written or delivered. Hence, in some cases, it happens that what now appear as successive chapters have in reality been composed at wide intervals of time, and addressed to audiences of very dissimilar character. Although, as stated, a certain amount of pruning has been done, there are occasional repetitions; possibly also a few inconsistent statements may be detected on comparing the earlier with the later essays—more, I trust, in matters of opinion than of fact. This is almost inevitable, unless some portions had been recast, or, to a greater or less extent, rewritten—which, as the essays are, to all intents and purposes, reprints, I did not feel justified in doing. It is to be expected that the wider knowledge which should follow upon many years of reading and study would modify, or possibly even altogether change, the impressions of the earlier time.

My thanks are due to the Proprietors and Editors of the *Contemporary* and *Fortnightly Reviews* and *Knowledge* for permission to include certain articles which have appeared in those periodicals. I am also indebted to the Council of the British Association for the Advancement of Science for permission to reprint the Presidential Address delivered to the Chemical Section of the Association at the meeting in Leeds in 1890. Messrs. Macmillan and the editor have allowed me to make use of certain articles contributed to *Nature*; the Managers of the Royal Institution have permitted me to reprint the lecture on Wöhler; the Council of the Chemical Society, those on Kopp, Victor Meyer, and Julius

Thomsen, and the Presidential Address of 1900; the Council of the Philosophical Society of Glasgow, the Graham Lecture, given in 1887; and the Council of the Greenock Philosophical Society, the Watt Anniversary Address of 1898. Lastly, I have to thank Mr. John Heywood, the publisher of the Manchester Science Lectures, for granting me permission to make use of the lectures on Priestley and Cavendish.

LONDON, *May* 1911.

CONTENTS

I

ROBERT BOYLE

	PAGE
(One of the Free Evening Lectures delivered in connection with the Loan Collection of Scientific Apparatus at South Kensington in 1876)	1

II

JOSEPH PRIESTLEY

(A Lecture delivered in the Hulme Town Hall, Manchester, on 18th November 1874. Manchester Science Lectures) .	32
--	----

III

CARL WILHELM SCHEELE

(An Address to the Owens College Chemical Society, at the Opening Meeting, 24th October 1893 ; subsequently published in the <i>Fortnightly Review</i>)	60
--	----

IV

HENRY CAVENDISH

(A Lecture delivered in the Hulme Town Hall, Manchester, on 24th November 1875. Manchester Science Lectures) .	79
--	----

CONTENTS

V

JAMES WATT

	PAGE
(Being the Watt Anniversary Lecture delivered before the Greenock Philosophical Society on 11th March 1898)	98

VI

ANTOINE-LAURENT LAVOISIER

(<i>Contemporary Review</i> , December 1890)	123
---	-----

VII

PRIESTLEY, CAVENDISH, LAVOISIER, AND *LA RÉVOLUTION CHIMIQUE*

(The Presidential Address to the Chemical Section of the British Association for the Advancement of Science, Leeds, 1890)	149
---	-----

ADDENDUM—

(M. Berthelot and the Address to the Chemical Section of the British Association at Leeds, 1890)	176
--	-----

VIII

MICHAEL FARADAY

(A Review of Dr. Bence Jones's "Life and Letters of Faraday."— <i>Manchester Guardian</i> , 1870)	185
---	-----

IX

THOMAS GRAHAM

(A Lecture (with Additions) delivered in the Yorkshire College, Leeds, introductory to the Evening Class Session, 1877-78)	206
The Third Triennial "Graham" Lecture, delivered before the Philosophical Society of Glasgow in Anderson's College, 16th March 1887	271

CONTENTS

xi

X

FRIEDRICH WÖHLER

	PAGE
(A Lecture delivered at the Royal Institution, Albemarle Street, on Friday Evening, 15th February 1884)	294

XI

JEAN BAPTISTE ANDRÉ DUMAS

(A Lecture delivered to the Royal Dublin Society, March 1885)	318
---	-----

XII

HERMANN KOPP

(Memorial Lecture delivered to the Fellows of the Chemical Society, 20th February 1893 ; published in the <i>Transactions of the Chemical Society</i> , 1893)	364
--	-----

XIII

VICTOR MEYER

(Memorial Lecture delivered to the Fellows of the Chemical Society, 8th February 1900 ; published in the <i>Transactions of the Chemical Society</i> , 1900)	423
---	-----

XIV

DMITRI IVANOWITSH MENDELEEFF

("Scientific Worthies," No. XXVI.— <i>Nature</i> , 27th June 1889)	483
--	-----

XV

STANISLAO CANNIZZARO

("Scientific Worthies," No. XXX.— <i>Nature</i> , 6th May 1897)	500
---	-----

XVI

JULIUS THOMSEN

	PAGE
(Memorial Lecture delivered to the Fellows of the Chemical Society, 17th February 1910; published in the <i>Transactions of the</i> <i>Chemical Society</i> , 1910)	515

XVII

THE RISE AND DEVELOPMENT OF SYNTHETICAL CHEMISTRY

(The Presidential Address to the Sutton Coldfield Institute, 1892; subsequently published in the <i>Fortnightly Review</i>)	533
---	-----

XVIII

ON THE PROGRESS OF CHEMISTRY IN GREAT BRITAIN AND
IRELAND DURING THE NINETEENTH CENTURY

(The Presidential Address to the Chemical Society, 29th March 1900; published in the <i>Transactions of the Chemical Society</i> , 1900)	551
--	-----

XIX

ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING
THE REIGN OF QUEEN VICTORIA

(An Address delivered at the East London Technical College, 8th February 1897; subsequently published in <i>Knowledge</i>)	590
--	-----

I

ROBERT BOYLE

ONE OF THE FREE EVENING LECTURES DELIVERED IN CONNECTION
WITH THE LOAN COLLECTION OF SCIENTIFIC APPARATUS AT SOUTH
KENSINGTON IN 1876.

FROM whatever point of view we may regard it, the period which began with the restoration of the House of Stuart and ended with its downfall is one of the most extraordinary in our history. It is a period of paradoxes. The reign of Charles II. is at once one of the worst and one of the brightest epochs in our annals. Never were the resources of this country so recklessly wasted; never was it more wretchedly governed. At home, public morality and political virtue were at their lowest ebb. Abroad, the foreign policy of the power which the firmness of Oliver had made to be everywhere respected was the subject of derision in even the smallest of German courts. The boys of Amsterdam, who, as Macaulay tells us, ran along the canals, when the great Protector was no more, shouting for joy that the Devil was dead, had as men the gratification of helping De Winter to burn our arsenals, and of insulting Tilbury, sacred to the memory of Elizabeth, and of one of the proudest moments of our national existence. On the other hand, at no former period were such mighty legislative reforms enacted; blow after blow was aimed at

and made its mark upon spiritual tyranny and territorial aggression ; and the Church was made to admit, with Praed's good old Dr. Brown,

That if a man's belief is bad,
It will not be improved by burning.

If our literature was debased by the ribaldry of a crowd of dramatists and poetasters, it was purified and ennobled by the sublime genius of Milton and the brilliant fancy of Dryden.

Times so rich in incongruities, the times of Clarendon, Halifax, Hale, Russell, Milton, and Jeremy Taylor ; and of Buckingham, Sunderland, Jeffreys, Oates, and the Duchess of Portsmouth, have been the wonder and the despair of historians. The inquiry how, under such untoward circumstances, such a marvellous progress was possible, was a riddle which it has been left to our own age to solve. This movement was the effect of that vague and indefinable force we call the spirit of the age ; and the spirit of that age, as it has been laid bare to us by the searching anatomy of the author of the *History of Civilisation in England*, was a sceptical, inquiring, reforming spirit. It pervaded every department of knowledge and of intellectual energy. It was rife in theology, in politics, in philosophy, and eventually in science. This spirit may be said to have infused itself in science with the appearance in 1661 of a little octavo volume from an Oxford printing press : it came forth without any preparatory bustle, anonymously and undedicated. But in its revolt against mere authority, in its disdain of old-world notions, and in its ill-concealed contempt for the schoolmen, it so exactly caught and expressed the spirit of the time that it instantly arrested the attention of the learned world, and not only

of the small world of the *virtuosi*, but of that infinitely larger public of thinking men who felt a growing impatience of the dogmas of the schools. The book was entitled the "Sceptical Chymist: or Chemico-Physical Doubts and Paradoxes touching the Experiments, whereby vulgar Spagyristes are wont to endeavour to evince their Salt, Sulphur, and Mercury to be the true Principles of Things." There was not much in such a title to attract the common public, nor were its merits as a piece of literary workmanship of a very high order; nevertheless, the book was eagerly bought up, and its popularity was such as to attract even the attention of foreigners visiting London, and no fewer than ten Latin editions of it appeared on the Continent. Who was the author? was in everybody's mouth. Men declared that the mantle of the great lawgiver who, as Cowley sung, had seen, as from the summit of Pisgah, the land which he was not permitted to enter, had fallen upon him. The writer was soon identified as a young gentleman, the youngest son of an Irish peer, who the year before had ventured abroad a treatise on the elastic power of the air, in which he exploded the notion of a *Fuga Vacui*, and for doing which he had drawn down upon himself the trenchant wrath of the author of the *Leviathan*—Hobbes of Malmesbury, the last man of note in England who did battle for the Plenists.

This young man was called the Honourable Robert Boyle: he was the seventh son and the fourteenth child of the Great Earl of Cork, and was born at Lismore, in the county of Waterford, on 25th January 1626. His father, Richard Boyle, a younger son of the younger branch of a Hertfordshire family which could trace its ancestry back to the times of Edward the Confessor, despairing of employment at home, had

resolved to push his fortunes in Ireland, and at twenty-two years of age found himself in Dublin with no other worldly possessions than a taffety doublet and a pair of black velvet breeches laced, a new Milan fustian suit, two cloaks and competent linen, a couple of tokens, a trusty rapier and dagger, and twenty-seven pounds three shillings in ready money. From these inconsiderable beginnings he built, as his son relates with pride, so plentiful and so eminent a fortune that his prosperity found many admirers but few parallels. Of the mother of Robert Boyle we learn little beyond that she was the daughter of Sir Geoffrey Fenton, Principal Secretary of State for Ireland, that she wanted not beauty, and was rich in virtue. She died when her youngest son was only a few years old, and he tells us that he ever counted it among the chief misfortunes of his life that he knew not her that gave it him. When eight years of age he was sent to Eton, at which time Sir Henry Wotton was Provost: a fine gentleman himself, and well skilled in the art of making others so. Here the studious sickly boy with his uncouth manners, his stuttering speech, and his roving habits, must have been sorely tried had he not fallen into good hands: as it was, Eton and Sir Henry were ever pleasant memories to him. It might have been otherwise, however, for through a stupid mistake of a careless apothecary he had nearly lost his life; which accident, he said, made him long after apprehend more from the physicians than from the disease, and was possibly the occasion that made him so inquisitively apply himself to the study of physic that he might have the less need of them that profess it. Years after he himself was nominated to the Provostship by Charles II., but his objection to take orders, in spite of the advice of

Clarendon, overcame his inclination to accept an office to which his habits and associations disposed him. At the age of twelve he was sent with an elder brother to the Continent, where he remained for six years. The good old earl his father died in the midst of the troubles occasioned by the insurrection in Ireland, and Boyle with great difficulty found his way back to England and to his manor at Stalbridge, in Dorsetshire, which had descended to him. Here he lived in great retirement throughout the unhappy times which culminated in the death of Charles I., seeking in his books and in his laboratory some diversion from the contemplation of the miseries of his country. At about this time Boyle became a member of what its promoters pleasantly termed the Invisible College, an assembly of learned and curious gentlemen who applied themselves to the study of experimental science, or, as it was then called, the New Philosophy. The little band included John Wallis, the mathematician; John Wilkins, afterwards Bishop of Chester; Jonathan Goddard, Warden of Merton; Samuel Foster, Professor of Astronomy at Gresham College; and Theodore Haak, a German resident in London, who appears to have first suggested the meetings. These were held weekly at each other's lodgings in London or at Gresham College, "to discourse and consider of philosophical inquiries and such as related thereunto (precluding matters of theology and State affairs)." A certain portion of the company removed to Oxford, and continuing the meetings, were joined by Seth Ward, afterwards Bishop of Salisbury; Ralph Bathurst, President of Trinity College, Oxford; Dr., afterwards Sir William, Petty; Thomas Willis, and others. Boyle followed in 1654, and thereafter the philosophers met at his lodging, with Crosse, an

apothecary, for the convenience of inspecting drugs. The Oxford section, however, always seemed to regard their Gresham brethren as constituting the parent society, and from time to time they journeyed up to London for the purpose of attending the meetings at the College. Out of such beginnings grew the Royal Society of London for Improving Natural Knowledge, incorporated by Charles II. in 1663; and in the charter Boyle is named as one of the council. The growth of the new philosophy excited the jealousy and anger of those who affected to see in the ascendancy of the Baconian method the subversion of everything that was orderly and of good repute. Religion, they cried, was being undermined; civil law was gone; the empire of reason and of all true learning was at an end. Bishops anathematised; Hobbes, who certainly had scant affection for the clergy, thundered; Butler lampooned. Boyle was earnestly solicited to leave the society. "I beseech you, sir," writes one of his correspondents, "consider the mischief it hath occasioned in this once flourishing kingdom, and if you have any sense, not only of the glory and religion, but even of the being of your native country, abandon that constitution. It is too much that you contribute to its advancement and repute: the only reparation you can make for that fatal error is to desert it betimes. Do not you apprehend that all the inconveniences that have befallen the land, all the debauchery of the gentry (which ariseth from that pious and prudent breeding, which was and ought to have been continued) will be charged on your account? . . . It will be impossible for you to preserve your esteem but by a seasonable relinquishing of these impertinents." The writer, Henry Stubbe, a physician of repute, was one of those unquiet spirits of

which the times were fertile : he was formerly a Student of Christ Church and Keeper in the Bodleian Library, where he wrote several tracts ; his *Essay on the Good Old Times* was pardonable, but his *Apology for the Quakers* was too much for the patience of the University, and he lost his position in Oxford. At the time of the outcry against the Royal Society Stubbe made himself the champion of his faculty, the majority of whom condemned Sydenham for believing that the new-fangled philosophy and physic might have something good in common, and he fell foul of Glanvill, Rector of Bath, who had followed Sprat in lauding the institution and objects of the society, in his characteristic fashion. The controversy raged with no little fury and bitterness, and hard knocks were freely given and returned, as was the manner of the time. Fate decreed, however, that Glanvill should have the last word, for his adversary being accidentally drowned near Bath, it fell to the rector's lot to preach his funeral sermon. And let us hope that he saw in it, as the French say, a grand opportunity for holding his tongue.

But the Royal Society, notwithstanding the rough usage of its youth, continued to grow and prosper, and people began even to see that it might be of use to them in their day and generation. The Great Plague of 1665 and the Great Fire of 1666 gave the Society an opportunity, and much of what was good in the arrangement in the new city was the result of their deliberations and counsel. Science became even fashionable. The King set up a laboratory, and amused himself by making weather observations with the newly invented baroscope ; the fine ladies of his court marvelled at the properties of the phosphorus (of so curious an origin) which Mr. Krafft had brought from Germany ; and the gentlemen

at Will's conversed of the *Vacuo Boyliano* and the spring of air. Moreover, many of the matters upon which the learned world at that time disputed were, when stated in intelligible terms, of common interest. One of the points about which people wrangled, when reduced to a plain issue, was this : Is a vacuum possible ; that is, can a space absolutely void of matter be obtained ? A few years before a very learned Frenchman, René Descartes, had asserted that, as according to his thinking the universe was absolutely full, it was impossible even to conceive of the existence of a vacuum ; and such was his subtlety and logic that many other learned persons came to be of his opinion. But when men began to use their hands and eyes as well as their reason in attempting to get at nature's secrets, doubts arose whether the explanations and hypotheses of the gown-men were not rather strained, and for the most part unsatisfactory. For example, the manner in which Mr. Hobbes explained the action of the watering-pot would scarcely commend itself to the readers of his *Dialogus Physicus de Natura Aeris* :—" If a gardener's watering-pot be filled with water, the hole at the top being stopped, the water will not flow out at any of the holes in the bottom ; but if the finger be removed to let in the air above, it will run out at them all, and as soon as the finger is applied to it again the water will suddenly and totally be stayed again from running out. The cause whereof seems to be no other but this, that the water cannot, by its natural endeavour to descend, drive down the air below it, because there is no place for it [the air] to go into, unless either by thrusting away the next contiguous air it proceed by continual endeavour to the hole at the top, where it may enter and succeed in the place of the water that floweth out,

or else by resisting the endeavour of the water downwards penetrate the same, and pass up through it."

Unfortunately for the Plenists, as Mr. Hobbes and the Cartesians came to be called, there were some awkward facts which did not seem to agree at all with the notion of the plenitude of the world. There was the fact that if a tube, say 35 feet long, closed at one end and open at the other, be completely filled with water and inverted with the open end under water, the level of the water in the tube will sink a few feet—that is, the water-column will not exceed some 32 or 33 feet in length, measured from the level of the liquid in the cistern. Moreover, if the experiment be repeated with quicksilver, which is more than thirteen times heavier than water, bulk for bulk, the height of the quicksilver column will be only one-thirteenth of that of the water. And there was this particularly awkward fact, that if the tube containing the quicksilver be carried up a high tower, as did Claudio Bereguardi up the leaning tower of Pisa, or up a mountain, as did Perrier some four or five years later in France, or as Mr. Richard Townley did up one of the Lancashire hills, the space above the mercury became much greater as the summit was approached, and became less again as the descent was made. Curious persons naturally asked why the mercury behaved in this way, and what was in the space above the level of the liquid? If the world were actually as full as an egg, the existence of these apparently empty spaces was certainly very perplexing. (It was not at all clear why nature should be so partial in her likings and dislikings as to put up with a much bigger space from the mercury than she would from the water, and it seemed rather irrational for her to hate a vacuum less at the top of a mountain than at the bottom.)

It was about this time that Boyle published in the form of a letter to his nephew, Lord Dungarvan, his "New Experiments Physico-Mechanical touching the Spring of Air and its Effects, made for the most part in a new Pneumatical Engine." Shortly after Boyle had turned his attention to physical science he heard of a book "published by the industrious Jesuit Schottus, wherein it was related how that that ingenious gentleman, Otto Gericke, Consul of Magdeburg, had lately practised in Germany a way of emptying glass vessels by sucking out the air at the mouth of the vessel plunged under water." Boyle at once recognised that important results might be expected to follow the study of the phenomena of the air's rarefaction, but he also saw that such results could scarcely be furnished by Von Guericke's method. He accordingly sought to devise a more perfect form of instrument, and with the assistance of Robert Hooke, a man of remarkable inventive powers, he, about the year 1658, contrived his "Pneumatical Engine."

It consisted of a large pear-shaped vessel holding about thirty wine-quarts, fitted with a stopper at the top and connected at the bottom with a brass cylinder in which was a piston worked by a rack and pinion. Between the glass vessel, "which we," says Boyle, "with the glassmen shall often call a receiver for its affinity to the large vessels of that name used by chemists," and the cylinder was a stopcock which was alternately opened and closed as the piston was worked up and down, the air from the cylinder being allowed to escape through a small hole at the top, temporarily closed by a stopper. The mode of working the pump will be obvious. "By the repetition of the motion of the sucker [piston] upward and downward, and by

opportunately turning the key [of the stopcock] and stopping the valve [the brass peg inserted into the cylinder] as occasion requires, more or less air may be sucked out of the receiver according to the exigency of the experiment and the intention of him that makes it."

Before describing his experiments in detail Boyle proceeds to "insinuate that notion by which it seems likely that most if not all of them will prove explicable, namely, that there is a spring or elastical power in the air we live in. By which *elater* or spring of the air, that which I mean is this: that our air either consists of, or at least abounds with, parts of such a nature, that in case they be bent or compressed by the weight of the incumbent part of the atmosphere, or by any other body, they do endeavour, as much as in them lieth, to free themselves from that pressure, by bearing against the contiguous bodies that keep them bent; and, as soon as those bodies are removed, or reduced to give them way by presently unbending and stretching out themselves, either quite, or so far forth as the contiguous bodies that resist them will permit, and thereby expanding the whole parcel of air these elastical bodies compose." Boyle pictured to himself this process of unbending and stretching by considering the air near the earth to be "such a heap of little bodies lying one upon another as may be resembled to a fleece of wool. For this (to omit other likenesses betwixt them) consists of many slender and flexible hairs; each of which may indeed, like a little spring, be easily bent or rolled up; but will also, like a spring, be still endeavouring to stretch itself out again. For though both these hairs, and the aerial corpuscles to which we liken them, do easily yield to external pressures; yet each of them (by virtue of its structure) is endowed with a power or principle of self-

dilatation ; by virtue whereof, though the hairs may, by a man's hand, be bent and crowded closer, and into a narrower room than suits best with the nature of the body ; yet, whilst the compression lasts, there is in the fleece they compose an endeavour outwards, whereby it continually thrusts against the hand that opposes its expansion. And upon the removal of the external pressure, by opening the hand more or less, the compressed wool doth, as it were, spontaneously expand or display itself towards the recovery of its more loose and free condition, till the fleece hath either regained its former dimensions, or at least approached them as near as the compressing hand (perchance not quite opened) will permit."

This passage illustrates in a remarkable manner the mechanical turn of Boyle's mind, and the extreme caution with which he invariably expressed his opinions. Humboldt, indeed, calls him "the cautious and doubting Robert Boyle." He was well aware that other modes of explaining the elasticity of the air were possible, and, in fact, he cites that of Descartes, that the air is nothing but a heap of small flexible particles raised by the sun's heat "into that fluid and subtle and ethereal body which surrounds the earth ; and by the restless agitation of that celestial matter, wherein those particles swim, are so whirled round that each corpuscle endeavours to beat off all others from coming within the little sphere requisite to its motion about its own centre ; and in case any, by intruding into that sphere, shall oppose its free rotation to expel or drive it away." The vehement agitation which the particles receive from the fluid æther that swiftly flows between and whirls about each of them, as the eddying stream about the corks, not only keeps them separated, but also makes them hit against

and knock away each other, and consequently require more room than they would need if compressed. After all, there is a certain resemblance in this to our modern notions of the constitution of a gas. On the whole, Boyle is inclined to his own hypothesis, but he is unwilling, as he says, "to declare peremptorily for either of them against the other"; for "to determine whether the motion of restitution in bodies proceed from this, that the parts of a body of a peculiar structure are put into motion by the bending of the spring, or from the endeavour of some subtle ambient body whose passage may be stopped or obstructed, or else its pressure unequally resisted by reason of the new shape or magnitude, which the bending of a spring may give the pores of it: to determine this, I say, seems to me a matter of more difficulty than at first sight one would easily imagine it."

Boyle had a perfectly clear conception of the materiality of air, and he attempted on several occasions to determine its weight, although it is remarkable that he who was so familiar with the principle of Archimedes that a body weighed in a fluid loses of its weight an amount equal to that of the bulk of fluid displaced, should have made the experiment by weighing bladders first empty and then inflated with air. Indeed, he himself was the first to show that a bladder containing air and counterpoised with metallic weights appears to weigh more *in vacuo* than in the air; and the familiar experiment in which the cork ball seems to increase in weight when placed in the exhausted receiver was first devised by him.

"Taking it for granted then," he goes on to say, "that the air is not devoid of weight, it will not be uneasy to conceive that that part of the atmosphere wherein we live, being the lower part of it, the corpuscles that compose

it are very much compressed by the weight of all those of the like nature that are directly over them ; that is, of all the particles of air that being piled up upon them, reach to the top of the atmosphere." He then recalls to mind the observation that the mercurial column in the barometer stands lower at the top of a mountain than at the bottom, "of which the reason seems manifestly enough to be this, that upon the tops of high mountains the air, which bears against the restagnant quicksilver, is less pressed by the less ponderous incumbent air." He next disposes of the possible objection that the air thus strongly compressed by the superincumbent atmosphere should yet yield readily to the motion even of little flies and feathers, by demonstrating "that it is the equal pressure of the air on all sides upon the bodies that are in it, which causeth the easy cession of its parts, which may be argued from hence ; that if by the help of our engine the air be but in great part, though not totally, drawn away from one side of a body without being drawn away from the other, he that shall think to move that body to and fro, as easily as before, will find himself much mistaken." To demonstrate this Boyle was wont to tie a partially inflated bladder to the stopper of the receiver, and to desire a bystander to lift up the stopper after the receiver was partly exhausted : it was "pleasant to see," he says, "how men will marvel that so light a body, filled at most with but air, should so forcibly draw down their hands, as if it were filled with some very ponderous thing." The distension of the bladder, in consequence of the expansion of the included air, as the rarefaction in the receiver proceeded, affords him an additional proof of the force of the spring of air. He proceeds to point out that the force of this spring may be augmented by heat : "the elastical power

of the same quantity of air may be as well increased by the agitation of the aerial particles (whether only moving them more swiftly and scattering them, or also extending or stretching them out, I determine not) within an every way inclosing and yet yielding body ; as displayed by the withdrawing of the air that pressed it without. For we found that a bladder but moderately filled with air and strongly tied, being awhile held near the fire, not only grew exceedingly turgid and hard, but afterwards being brought nearer to the fire suddenly broke with so loud and vehement a noise as stunned those that were by, and made us for a while after almost deaf." The connection of these phenomena singularly impressed Boyle ; and he says that it deserves "deliberate speculation." During the two centuries which have elapsed since then many men have given this matter a vast amount of "deliberate speculation," with the result of showing that this connection is even more intimate than Boyle, with all his prevision, could have dreamt of.

The relation of the air to combustion, and the nature of flame, attracted much attention from Boyle, and he frequently returned to these subjects in the course of his work. His observations on the burning of candles in a partial vacuum are worth mention for the evidence they afford of the care with which he noted even the minutest phenomena attending an experiment. After proving that the flame is extinguished long before the exhaustion is complete, he goes on to say "that these things were further observable, that after the two or three first exsuctions of the air, the flame (except at the very top) appeared exceedingly blue, and receded more and more from the tallow, till at length it appeared to possess only the very top of the wick, and there it went out." These phenomena, apparently so trivial, are now recog-

nised as of importance in connection with the theory of illuminating flames.

Boyle next proceeds to what he evidently regards as a great *experimentum crucis*, "whereof," he says, "the satisfactory trial was the principal fruit I promised myself from our engine": it related to the behaviour of a barometer in the exhausted receiver. After carefully fitting the barometer into the receiver, so that the outer air could not press down upon the surface of the metal in the cistern, he drew down the sucker, and found to his delight that the mercury fell within the tube and continued to fall so long as the pump was worked until it was only an inch or so from the level of that in the cistern: on readmitting the air the mercury was impelled up again to its original position in the tube. The importance of this observation was obvious, and all Oxford came to see an experiment which afforded such a signal confirmation of the truth of the speculations of Galileo and Pascal. It now occurred to Boyle to try what relation existed between the height of the mercurial column and the number of suctions made by the pump, for he had observed that the first sucks caused a far more rapid decrease in the height than the last. Boyle, we see, is now on the verge of the great discovery which has made his name familiar to every schoolboy in this country. It is worth noticing that it was in all probability the accident of the mode of construction of his engine, and the fact that each suction drew out a determinate bulk of air, that induced him to attempt to determine the relation between the pressure and volume of the air. He was forced, however, to abandon the attempt at this time, for he found that with the apparatus in its present form he was unable to make observations accurate enough to reduce them to

any hypothesis. "Yet," he adds, "would we not discourage any from attempting it, since if it could be reduced to a certainty it is probable that the discovery would not be unuseful."

He is now forced to confront the ever-recurring question—Is there a vacuum? and accordingly he proceeds to take the arguments of the Plenists to pieces. What proof, he asks, do they offer of the existence of that subtle ethereal matter which they say must exist in the space above the mercury. Why must exist? Because, they answer, there cannot be a void. And there cannot be a void because extension is the only nature of a body, and to say a space is devoid of body is, to the schoolmen, a contradiction *in adjecto*. The matter is, in fact, reduced to a question of metaphysics, and Boyle gives it up, "finding it very difficult either to satisfy naturalists with this Cartesian notion of a body, or to manifest wherein it is erroneous." The truth is, Boyle was hampered by his corpuscular notions, or he would assuredly have gone over to the Vacuists. He puts his candles and his bladders into his receivers, and however completely he may pump out the air, the things are none the less visible, and he asks—Can it be seriously imagined that light can be conveyed from an object without some vehicle to convey it?

He then substituted water for mercury, and repeated the experiment. As the rarefaction proceeded, he was struck with the appearance of a multitude of air bubbles within the liquid. The origin of this air puzzled him greatly. Was the water turned into air, or was the air pre-existent and latent in the water? On the whole, he inclines to the latter supposition, but mainly for the reason that all experience showed that water was elementary, indestructible, and inconvertible. He argues

the matter at such length that he is constrained to apologise for his prolixity in treating of such empty things as bubbles; yet he does not fail to point the moral "that there are very many things in nature that we disdainingly overlook as obvious or despicable, each of which would exercise our understandings, if not pose them too, if we would but attentively enough consider it, and not superficially contemplate it, but attempt satisfactorily to explicate the nature of it."

The idea that the air was the medium by which sound is ordinarily conveyed was familiar enough to the philosophers of the seventeenth century, and Boyle furnishes a proof of the fact by the observation that the ticking of a watch placed in the receiver became inaudible when the air was withdrawn.

The mode of action of the syphon next engages his attention, and he proceeds to inquire what must be the height of the atmosphere on the assumption that it has the same density at all points that it possesses on the earth's surface. He has completed the proof that the pressure of the air supported the mercurial column; his problem was to determine how much heavier mercury is than air, bulk for bulk; he would thus be able to calculate the height of a column of air, of the density of that on the earth's surface, required to balance a mercurial column of equal base and of 30 inches in height. Boyle unfortunately considered that the ratio of the weights of equal bulks of water and air was known with sufficient accuracy in his day, and after a discussion of all the observations with which he was acquainted, he concludes that water may be considered to be 1000 times heavier than air, which we now know to be greatly in excess of the truth. He proceeds, then, to inquire how much heavier mercury is than water, but

the observations of his predecessors on this point are so discordant that he feels himself obliged to re-determine the relation, firstly by observing the heights of counter-balancing columns of mercury and water in a U-shaped tube, and, secondly, by the method now adopted as the most accurate of all modes of estimating the specific gravities of liquids. By the first method he found that mercury is 13·7, by the second 13·68 times heavier than water: no very great disparity from the number 13·6 which we now adopt. From these data Boyle calculated that the atmosphere must be between six and seven miles high, on the supposition that it has the same density throughout that it has on the surface of the earth: in reality, on the same assumption, it is only between five and six miles high. Boyle was perfectly aware that this result was, in a sense, fictitious, but he shows that it was not without value as demonstrating what must be the minimum height of the atmosphere: it proved that the conjectures of Kepler and others that the air could not extend beyond a couple of miles or so from the earth's surface were certainly erroneous.

The main fact that air is related to life was of course as well understood in those days as it is now, but very little was known of the theory of respiration. Boyle made many experiments with his air-engine to elucidate this matter, and I am really afraid, in these anti-vivisection days, to tell you how many cats, mice, sparrows, fishes, tadpoles, and snails fell victims to his zeal. Not that he inflicted needless suffering, for Boyle was the most tender-hearted of men; if he has occasion to confine a mouse all night in one of his receivers, he places him near the fire, and consoles him with a bit of cheese, that he may be as comfortable as circumstances will permit; a lusty and pugnacious sparrow makes such a resolute

stand for existence that Boyle is fain to let him go; and the intercession of a lady is quite sufficient to deprive a certain kitten of the honour and glory of settling an important query concerning respiration.

The last experiment that Boyle describes is one of the most important and striking in the whole series, since by means of it he demonstrated how dependent is the boiling-point of a liquid upon the atmospheric pressure. Having boiled some water "a pretty while that by the heat it might be freed from the latitant air," he placed it, whilst still hot, within the receiver, when, on exhaustion, it again began to boil "as if it had stood over a very quick fire. . . . Once, when the air had been drawn out, the liquor did, upon a single exsuction, boil so long with prodigiously vast bubbles that the effervescence lasted almost as long as was requisite for the rehearsing of a *Pater Noster*. This experiment," he says, "seems to teach that the air by its stronger or weaker pressure may very much modify (as the school-men speak) divers of the operations of that vehement and tumultuous agitation of the small parts of bodies, wherein the nature of heat seems chiefly, if not solely, to consist."

Such is a very rapid and a very imperfect summary of this great work. I have purposely quoted very largely from it, for I wished to show you, in Boyle's own words, how wonderfully near much of the philosophy of the seventeenth century is to that which we are too apt to regard as the outcome of the nineteenth. It is impossible to exaggerate the importance of Boyle's labours; they served to give a marvellous sharpness to the notions of that time concerning the materiality of the air and of the phenomena which depend upon its elasticity. The work exhibits in an eminent degree Boyle's character as

an investigator, his quick perception and receptive mind, his great power of co-ordination, his insight, his logic, his patient care and scrupulous accuracy. It exhibits, too, his weakness; for it must be admitted that it is wanting in that grasp of principle and faculty of generalisation which we see in the work of the illustrious author of the *Novum Organum*. It lacks, too, the *Forscherblick* and power of divination so characteristic of the genius of Newton. But to say that Boyle is only inferior to Bacon and Newton is to assign him one of the first niches in the Walhalla of the heroes of science.

But Boyle's work, as I have before hinted, was not allowed to go forth unchallenged; and the Elaterists were quickly taken to task, on the one hand by one Franciscus Linus, and on the other by a far more important personage—Thomas Hobbes, of Malmesbury. Hobbes has been styled the subtlest dialectician of his time, and a master of precise and luminous language; too frequently, however, that language lost more in elegance than it gained in force. Hobbes, although not a professed Peripatetic or a Cartesian, was a very pronounced Plenist. He utterly failed to see any virtue in the new philosophy, and the disparagement of the Gresham set, or "the experimental philosophers," as he sneeringly called them, was the chief design of his *Dialogus Physicus de Natura Aeris*, the book in which he attempts to write down Boyle and his work. Boyle hated contention; but he and his friends felt that the new doctrines were at stake. It is unnecessary for me to take up your time by examining Mr. Hobbes's arguments or Boyle's refutation of them; it is sufficient to say that Mr. Hobbes, who had, with singular indiscretion, laid himself open by quoting Vespasian's law, "*That it is unlawful to give ill language first, but civil and*

lawful to return it," was taught politeness and much sound philosophy. The world will willingly let the *Dialogus* die, or remember it only in connection with Boyle's *Examen* of it.

We cannot, however, so summarily dismiss Franciscus Linus. Linus sets out to prove that the mercury in the Torricellian experiment is upheld not by the pressure of the air but by a certain nondescript internal cord; and Boyle undertakes to show that this hypothesis of an internal *funiculus*, which he remarks, with quiet humour, "seems to some more difficult to conceive than any of the phenomena in controversy is to be explained without it, is 'partly precarious, partly unintelligible, partly insufficient, and besides needless.'" Indeed the matter is scarcely worth mention except for the circumstance that it gave an occasion to Boyle to return to the question, which we have seen had already interested him, of the relation between the volume and the pressure of the air. In the answer to Linus he gives two new experiments touching the measure of the force of the spring of air compressed and dilated. The account of these memorable experiments must be given in Boyle's own words: "We took then a long glass tube, which, by a dexterous hand and the help of a lamp, was in such a manner crooked at the bottom, that the part turned up was almost parallel to the rest of the tube, and the orifice of this shorter leg of the syphon (if I may so call the whole instrument) being hermetically sealed, the length of it was divided into inches (each of which was subdivided into eight parts) by a straight list of paper, which, containing those divisions, was carefully pasted all along it. Then putting in as much quicksilver as served to fill the arch or bended part of the syphon, that the mercury standing in a level might reach in the

one leg to the bottom of the divided paper, and just to the same height or horizontal line in the other, we took care, by frequently inclining the tube, so that the air might freely pass from one leg into the other by the sides of the mercury (we took, I say, care), that the air at last included in the shorter cylinder should be of the same laxity with the rest of the air about it. This done, we began to pour quicksilver into the longer leg of the syphon, which, by its weight pressing up that in the shorter leg, did by degrees straighten the included air; and continuing this pouring in of quicksilver till the air in the shorter leg was by condensation reduced to take up but half the space it possessed (I say possessed, not filled) before, we cast our eyes upon the longer leg of the glass, upon which we likewise pasted a list of paper carefully divided into inches and parts, and we observed, not without delight and satisfaction, that the quicksilver in that longer part of the tube was 29 inches higher than the other. Now that this observation does both very well agree with and confirm our hypothesis, will be easily discerned by him that takes notice what we teach: and Monsieur Pascal and our English friend's [Mr. Townley's] experiments prove, that the greater the weight is that leans upon the air, the more forcible is its endeavour of dilatation, and consequently its power of resistance (as other springs are stronger when bent by greater weights). For this being considered, it will appear to agree rarely well with the hypothesis, that as according to it the air in that degree of density, and correspondent measure of resistance, to which the weight of the incumbent atmosphere had brought it, was unable to counterbalance and resist the pressure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment; so here the same air being

brought to a degree of density about twice as great as that it had before, obtains a spring twice as strong as formerly. As may appear by its being able to sustain or resist a cylinder of 29 inches in the longer tube, together with the weight of the atmospherical cylinder that leaned upon those 29 inches of mercury; and, as we just now inferred from the Torricellian experiment, was equivalent to them."

At this stage of the experiments the tube broke, and it was only after several mischances that Boyle was able to complete his observations.

He then proceeded to the converse experiment—that is, to determine the spring of rarefied air. A tube, about 6 feet in length, and sealed at one end, was nearly filled with mercury, and into it was placed "a slender glass pipe of about the bigness of a swan's quill, and open at both ends; all along of which was pasted a narrow list of paper, divided into inches and half-quarters. This slender pipe being thrust down into the greater tube almost filled with quicksilver, the glass helped to make it swell to the top of the tube; and the quicksilver getting in at the lower orifice of the pipe filled it up till the mercury included in that was near about a level with the surface of the surrounding mercury in the tube. There being, as near as we could guess, little more than an inch of the slender pipe left above the surface of the restagnant mercury, and consequently unfilled therewith, the prominent orifice was carefully closed with sealing-wax melted; after which the pipe was let alone for a while that the air, dilated a little by the heat of the wax, might, upon refrigeration, be reduced to its wonted density. And then we observed, by the help of the above-mentioned list of paper, whether we had not included somewhat more or

somewhat less than an inch of air; and in either case we were fain to rectify the error by a small hole made (with a heated pin) in the wax, and afterwards closed up again. Having thus included a just inch of air, we lifted up the slender pipe by degrees, till the air was dilated to an inch, an inch and a half, two inches, etc., and observed in inches and eighths the length of the mercurial cylinder, which, at each degree of the air's expansion, was impelled above the surface of the restagnant mercury in the tube. The observations being ended, we presently made the Torricellian experiment with the above-mentioned great tube of 6 feet long, that we might know the height of the mercurial cylinder for that particular day and hour, which height we found to be $29\frac{3}{4}$ inches."

Such were the experiments, simple and easily made, which led Boyle to the recognition of the great law which bears his name—a law which is so far from being "unuseful" that it is recognised by the physicist as of the first importance. And yet in spite of the thoroughness with which Boyle did the work, and in spite, too, of the precision with which he stated his results, the attempt has not been wanting to deprive him of the whole merit of this discovery, and there is scarcely a text-book of physics or chemistry on the Continent, or at least in France, in which his name is mentioned in connection with the matter: abroad they prefer to ascribe the glory to the Abbé Mariotte, although Mariotte's treatise, *De la Nature de l'Air*, in which he enunciates the law, was not printed until seventeen years after Boyle had published his reply to Linus.

"The results of the two series of experiments here detailed are given in the following tables:—

A TABLE OF THE CONDENSATION OF THE AIR

A	A	B	C	D	E
48	12	00		$29\frac{2}{16}$	$29\frac{2}{16}$
46	$11\frac{1}{2}$	$01\frac{7}{16}$		$30\frac{9}{16}$	$30\frac{9}{16}$
44	11	$02\frac{13}{16}$		$31\frac{15}{16}$	$31\frac{15}{16}$
42	$10\frac{1}{2}$	$04\frac{6}{16}$		$33\frac{8}{16}$	$33\frac{1}{7}$
40	10	$06\frac{3}{16}$		$35\frac{5}{16}$	35
38	$9\frac{1}{2}$	$07\frac{14}{16}$		37	$36\frac{15}{16}$
36	9	$10\frac{2}{16}$		$39\frac{5}{16}$	$38\frac{7}{8}$
34	$8\frac{1}{2}$	$12\frac{8}{16}$		$41\frac{11}{16}$	$41\frac{2}{3}$
32	8	$15\frac{1}{16}$		$44\frac{3}{16}$	$43\frac{11}{16}$
30	$7\frac{1}{2}$	$17\frac{15}{16}$		$47\frac{1}{16}$	$46\frac{3}{5}$
28	7	$21\frac{3}{16}$		$50\frac{5}{16}$	50
26	$6\frac{1}{2}$	$25\frac{9}{16}$		$54\frac{5}{16}$	$53\frac{10}{16}$
24	6	$29\frac{11}{16}$		$58\frac{13}{16}$	$58\frac{3}{8}$
23	$5\frac{3}{4}$	$32\frac{3}{16}$		$61\frac{5}{16}$	$60\frac{18}{16}$
22	$5\frac{1}{2}$	$34\frac{15}{16}$		$64\frac{1}{16}$	$63\frac{6}{11}$
21	$5\frac{1}{4}$	$37\frac{15}{16}$		$67\frac{1}{16}$	$66\frac{4}{7}$
20	5	$41\frac{9}{16}$		$70\frac{11}{16}$	70
19	$4\frac{3}{4}$	45		$74\frac{2}{16}$	$73\frac{11}{16}$
18	$4\frac{1}{2}$	$48\frac{12}{16}$		$77\frac{14}{16}$	$77\frac{7}{8}$
17	$4\frac{1}{4}$	$53\frac{11}{16}$		$82\frac{12}{16}$	$82\frac{4}{7}$
16	4	$58\frac{2}{16}$		$87\frac{14}{16}$	$87\frac{3}{8}$
15	$3\frac{3}{4}$	$63\frac{15}{16}$		$93\frac{1}{16}$	$93\frac{1}{8}$
14	$3\frac{1}{2}$	$71\frac{5}{16}$		$100\frac{7}{16}$	$99\frac{7}{8}$
13	$3\frac{1}{4}$	$78\frac{11}{16}$		$107\frac{13}{16}$	$107\frac{7}{18}$
12	3	$88\frac{7}{16}$		$117\frac{9}{16}$	$116\frac{4}{8}$

Added to 20 $\frac{1}{8}$ makes

AA The number of equal spaces in the shorter leg that contain the same parcel of air diversely extended.

B The height of the mercurial cylinder in the longer leg that compressed the air into those dimensions.

C The height of the mercurial cylinder that counterbalanced the pressure of the atmosphere.

D The aggregate of the two last columns, B and C, exhibiting the pressure sustained by the included air.

E What that pressure should be according to the hypothesis that supposes the pressure and expansion to be in reciprocal proportion.

A TABLE OF THE RAREFACTION OF THE AIR

A	B	C	D	E
1	00		29 $\frac{3}{4}$	29 $\frac{3}{4}$
1 $\frac{1}{2}$	10 $\frac{1}{2}$		19 $\frac{1}{2}$	19 $\frac{1}{2}$
2	15 $\frac{1}{2}$		14 $\frac{1}{2}$	14 $\frac{1}{2}$
3	20 $\frac{1}{2}$		9 $\frac{1}{2}$	9 $\frac{1}{2}$
4	22 $\frac{1}{2}$		7 $\frac{1}{2}$	7 $\frac{1}{2}$
5	24 $\frac{1}{2}$		5 $\frac{1}{2}$	5 $\frac{1}{2}$
6	24 $\frac{1}{2}$		4 $\frac{1}{2}$	4 $\frac{1}{2}$
7	25 $\frac{1}{2}$		4 $\frac{1}{2}$	4 $\frac{1}{2}$
8	26 $\frac{1}{2}$		3 $\frac{1}{2}$	3 $\frac{1}{2}$
9	26 $\frac{1}{2}$		3 $\frac{1}{2}$	3 $\frac{1}{2}$
10	26 $\frac{1}{2}$		3 $\frac{1}{2}$	2 $\frac{1}{2}$
12	27 $\frac{1}{2}$		2 $\frac{1}{2}$	2 $\frac{1}{2}$
14	27 $\frac{1}{2}$		2 $\frac{1}{2}$	2 $\frac{1}{2}$
16	27 $\frac{1}{2}$		2 $\frac{1}{2}$	1 $\frac{1}{2}$
18	27 $\frac{1}{2}$		1 $\frac{1}{2}$	1 $\frac{1}{2}$
20	28 $\frac{1}{2}$		1 $\frac{1}{2}$	1 $\frac{1}{2}$
24	28 $\frac{1}{2}$		1 $\frac{1}{2}$	1 $\frac{1}{2}$
28	28 $\frac{1}{2}$		1 $\frac{1}{2}$	1 $\frac{1}{2}$
32	28 $\frac{1}{2}$		1 $\frac{1}{2}$	0 $\frac{1}{2}$

Subtracted from 29 $\frac{3}{4}$ leaves

A The number of equal spaces at the top of the tube that contained the same parcel of air.

B The height of the mercurial cylinder that, together with the spring of the included air, counterbalanced the pressure of the atmosphere.

C The pressure of the atmosphere.

D The complement of B to C exhibiting the pressure sustained by the included air.

E What that pressure should be according to the hypothesis.

It would be quite impossible for me, in the time which remains, to attempt to go over, however superficially, the whole ground of Boyle's work, although there is much in it of special interest at the present time, as, for example, his papers on the *Saltiness of the Sea*, and the *Nature of the Sea's Bottom*; and his *Essay of the Intestine Motions of the Particles of Quiescent Solids wherein the absolute Rest of Bodies is called in question*. He was perhaps the earliest to

draw attention to the desirability of studying the forms of crystals, and his paper on the *Figures of Salts* contains many curious observations; in his *Experiments about the Superficial Figures of Fluids, especially of Liquors contiguous to other Liquors*, he breaks ground which has taxed the energies of our greatest mathematicians. His *Treatise on Cold* abounds with striking and original experiments: thus he demonstrates the expansive power of freezing water by bursting a gun-barrel filled with water and securely plugged, by placing it in a mixture of snow and salt, a freezing mixture which he himself brought into use in England. His *Essays on the Usefulness of Experimental Natural Philosophy* were of the greatest service in his time in furthering the cause of science by showing how the material interests of civilisation may be promoted by its study; and, lastly, his tract on *Unsuccceding Experiments* must have been as the wine of gladness and the oil of consolation to many a despondent virtuoso. His fame and his social position made Boyle's personal influence very considerable, and his house (or rather that of his sister, with whom he lived, for he was never married) was constantly besieged by a crowd of patentees and inventors, who sought his aid in bringing their schemes to the notice of the Government or the King: he was thus the means of introducing into the marine a method of obtaining fresh water from seawater, not very dissimilar to that which we owe to the late Dr. Normandy: this method, I need scarcely add, is not that of the ingenious youth who (whisper it not in the shades of Burlington Gardens!) gravely proposed to obtain fresh water from salt water by letting it stand and skimming it!

Boyle was a religious man, in the best sense of that

term, and his theological writings form no inconsiderable portion of his works. But we fear that Carneades and Eleutherius have made more stir, and, possibly, have done not less good in the world, than Lindamor and Eusebius. The *Christian Virtuoso* and the *Seraphic Love*, and possibly Swift's merciless *Pious Meditation on a Broomstick in the style of the Honourable Mr. Boyle*, have done more to perpetuate the *Occasional Reflections* than the *Occasional Reflections* have done for themselves.

Boyle was born in the year in which Bacon died : and Boyle's place in the history of science is that of the first true exponent of the Baconian method, and the *Sceptical Chymist* is his greatest work. This book probably contains a greater number of well-authenticated facts than is to be found in any other chemical treatise of its day. Many of these originated with Boyle, as, for example, the isolation of methyl alcohol from the products of the destructive distillation of wood, and that of acetone, which he prepared by heating the acetates of lead and lime.

But the greater merit of this work consists in its determined attack on the authority of the Peripatetics and the Paracelsians. Not that he is blind to the services of the Spagyrist : "the devisers and embracers of the hypothesis of the *tria prima* have done the commonwealth of learning some service by helping to destroy that excessive esteem or rather veneration, wherewith the doctrine of the four elements was almost as generally as undeservedly understood ! The Peripatetics, thinking it more high and philosophical to discover truth *a priori* than *a posteriori*, scorn the experimental method as descending to the capacities of such as can only be taught by their senses : the

dialectical subtleties of the schoolmen much more declare the wit of him that uses them than increase the knowledge or remove the doubts of sober lovers of truth." Boyle is very severe upon the affected mysticism of the Spagyrist. They may be as obscure as they like about their elixir, and the rest of their grand arcana, "yet when they pretend to teach the general principles of natural philosophers, this equivocal way of writing is not to be endured. For in such speculative inquiries where the naked knowledge of the truth is the thing principally aimed at, what does he teach me worth thanks, that does not, if he can, make his notion intelligible to me, but by mystical terms and ambiguous phrases darkens what he should clear up, and makes me add the trouble of guessing at the sense of what he equivocally expresses, to that of learning the truth of what he seems to deliver." Boyle indeed does not scruple to say that the reason why the Spagyrist wrote so obscurely of their three great principles was, that not having clear and distinct notions of them themselves, they could not write otherwise than confusedly of what they had confusedly apprehended: they could scarcely keep themselves from being confuted but by keeping themselves from being clearly understood—home-thrusts which must have made many a Helmontian wince. The effect of such hard hitting is made evident on the most superficial comparison of the general style of chemical treatises immediately preceding Boyle's time with those published towards the close of the seventeenth century.

The *Sceptical Chymist* sealed the fate of the doctrine of the *tria prima*, and before the close of the century the Paracelsians were as much out of date as a Phlogistian would be to-day. Boyle indeed seems to incline

to the belief that all matter is compounded of one primordial substance—in other words, that all matters are merely modifications of the *materia prima*—and how closely he was in accord with the modern spirit is manifest in this remarkable passage: “I am apt to think that men will never be able to explain the phenomena of nature, while they endeavour to deduce them only from the presence and proportions of such or such material ingredients, and consider such ingredients or elements as bodies in a state of rest; whereas indeed the greatest part of the affections of matter, and consequently of the phenomena of nature, seem to depend upon the motion and contrivance of the small parts of bodies.”

II

JOSEPH PRIESTLEY

A LECTURE DELIVERED IN THE HULME TOWN HALL, MANCHESTER, ON
18TH NOVEMBER 1874. MANCHESTER SCIENCE LECTURES.

THOSE of you who read newspapers will, probably, not have forgotten that on the 1st of August of this present year (1874) a great gathering took place at Birmingham to do honour to Joseph Priestley, one of that band of scientific worthies which made the reign of George III. memorable in the annals of science. On that day Professor Huxley (than whom no one is better qualified to appreciate the whole outcome of Priestley's life, or better able to set forth the singular force and beauty of his character) uncovered a statue which the friends of science and of liberal thought had raised to the memory of the philosopher. Birmingham, however, was not the only town in England, nor were Englishmen the only people, that did homage to the memory of Priestley on that day. The lovers of science in Leeds, near to which place he was born, assembled in public meeting; and the chemists of America, to which country he was driven by the political and theological bigotry of his own people, met together at his grave in a quiet little town on the banks of the Susquehanna river.

My object this evening is to give you some account of the labours of this philosopher, whose services in the

cause of truth, and whose sacrifices in the struggle for freedom of thought, were, seventy years after his death, thus gratefully recognised.

But the very richness of my material is a source of embarrassment; for Priestley was a man of so many and such diverse acquirements—

A man so various, that he seemed to be
Not one, but all mankind's epitome ;

his energy and power of application were so great, the range of his work so wide, that to attempt to do full justice to the many-sidedness of the man and of his labours would require me to inflict on you, not one lecture alone, but a whole series. You may form some conception of his marvellous mental activity, when I tell you that, as appears from the catalogue drawn up by his son after his death, he published no fewer than 108 works. Among them we have two volumes *On the History and Present State of Discoveries relating to Vision, Light, and Colours*; next, two volumes of *Disquisitions relating to Matter and Spirit*; *A Course of Lectures on Oratory and Criticism*; *A General History of the Christian Church*, in six volumes; *The Doctrine of Phlogiston Established*; *A Treatise on Civil Government*; six volumes of *Experiments on Different Kinds of Air*; *A Harmony of the Evangelists in Greek*; *A Familiar Introduction to the Theory and Practice of Perspective*; and *The Rudiments of English Grammar, Adapted to the Use of Schools*. And this formidable development of the *cacoëthes scribendi* came, as he tells us, by a practice of abstracting sermons and writing much in verse.

Some particulars of the life of this extraordinary man may be interesting to you. He was born in 1733,

at Fieldhead, a hamlet of some half-dozen houses, about six miles from Leeds. The old home of the Priestleys was pulled down some years ago. It was described by one who pointed out its site to me, and who remembered it well, as a little house of three small rooms, built of stones and slated with flags. Jonas Priestley, the father, was a cloth-dresser by trade. Of the mother but little is known beyond that she was the daughter of a farmer living near Wakefield. She died when Priestley was only seven years old, and he was taken charge of by his aunt, a Mrs. Keighley, a pious and excellent woman, in a good position, but who, as he tells us, "knew no other use of wealth, or of talents of any kind, than to do good." The boy was of a weakly consumptive habit, one consequence of which was seen in the desultory character of his early education. But his home-life with his aunt must have done much to make up for the deficiencies of his school training. She encouraged him in his fondness for books, and as her house was the resort of all the dissenting clergymen in the district without distinction, young Priestley was constantly brought in contact with men of culture and of liberal thought, and several of them seem to have made a lasting impression on his vigorous mind. Still, the gloomy Calvinism under which he was brought up, and the frequent talk of *experiences* and of *new births* to which he listened, had its effect upon the sensitive mind in the weakly frame. Years afterwards he wrote of this period: "I felt occasionally such distress of mind as it is not in my power to describe, and which I still look back upon with horror. Notwithstanding I had nothing very material to reproach myself with, I often concluded that God had forsaken me, and that mine was like the case of Francis Spira, to whom, as he imagined,

repentance and salvation were denied. In that state of mind I remember reading the account of the man in the iron cage in *The Pilgrim's Progress* with the greatest perturbation." But the strengthening intellect was not slow to recover its ascendancy; and Priestley could afterwards write, in his characteristic way of always looking at the sunny side of every circumstance: "I even think it an advantage to me, and am truly thankful for it, that my health received the check that it did when I was young; since a muscular habit from high health, and strong spirits, are not, I think, in general accompanied with that sensibility of mind which is both favourable to piety and to speculative pursuits."

Priestley was destined by his aunt for the ministry, but her views—which were his also—were for a time interfered with by his continued ill-health. Eventually he was sent to the Dissenting Academy at Daventry, which the labours of the good and learned Dr. Doddridge had brought into repute. Of the three years he spent there Priestley ever spoke with peculiar satisfaction. The system of study was congenial to his independent and inquisitive mind, for the freest inquiry on every article of theological orthodoxy and heresy was warmly encouraged, and every vexed question was in turn handled by the teachers, who took opposite sides in controversy, and incited their students to discussion. If training such as this laid the foundation of the successes of Priestley's after-life, it was also, and in no less degree, the source of much of his misfortune. His first charge, on leaving Daventry, was at Needham Market, in Suffolk; but his congregation did not like his Arianism, nor the stuttering way in which he told them of it, and they almost deserted him. Driven to extremities, he issued proposals to teach the classics and

mathematics for half a guinea a quarter, and to board the pupils in his house for twelve guineas a year. This scheme not answering, he next turned his attention to popular science, and commenced with a course of twelve lectures on "The Use of the Globes," from which he barely got enough to pay for his globes. Although he keenly felt the effects of what he terms his "low despised situation," Priestley never lost heart or hope. He could even say of his impediment in speech, that, like St. Paul's "thorn in the flesh," it was not without its use. "Without some such check as this," he writes, "I might have been disputatious in company, or might have been seduced by the love of popular applause as a preacher; whereas my conversation and my delivery having nothing in them that was generally striking, I hope I have been more attentive to qualifications of a superior kind."

Years afterwards, on being invited to preach in the district when he had raised himself to some degree of notice in the world, the same people crowded to hear him; and though his elocution was not much improved, they professed to admire one of the same discourses they had formerly despised.

From Needham he passed on to Nantwich, in Cheshire, where he found himself in more congenial society, and in better circumstances, so that he was able to buy books and a few philosophical instruments. Not that philosophy here occupied the whole of his leisure, for he tells us that he betook himself to music, and learned to play on the English flute, as the easiest instrument. Music he recommends to all studious persons; and it will be better for them, he says, if, like himself, they should have no very fine ear or exquisite taste, as by this means they will be more easily pleased,

and be less apt to be offended when the performances they hear are but indifferent. In 1761 he was invited to Warrington as "tutor in the languages" in the Dissenting Academy in that town. Here he taught Latin, Greek, Hebrew, French, and Italian; and delivered courses of lectures on Logic, on Elocution, on the Theory of Language, on Oratory and Criticism, on History and General Policy, on Civil Law, and on Anatomy. About this time, too, he made the friendship of Benjamin Franklin—a friendship which constitutes a turning-point in Priestley's career, for Franklin encouraged his leaning towards philosophical pursuits, warmly recommending him to undertake his proposed History of Electricity, and furnishing him with books for the purpose. In connection with this work, he made a number of original observations in electricity, on account of which the book was favourably received; its author was made a Fellow of the Royal Society, and a Doctor of Laws of Edinburgh University. Priestley by this time was married, but seeing no prospect of providing for his family at Warrington, he accepted an invitation to take charge of a congregation in Leeds, and thither he removed in 1767. Having leisure, he redoubled his attention to experimental philosophy, and began that brilliant series of discoveries by which others were to accomplish the overthrow of that system of chemical philosophy of which he considered himself the special champion. "But," writes Priestley, "the only person in Leeds who gave much attention to my experiments was Mr. Hey, a surgeon. . . . When I left Leeds he begged off me the earthen trough in which I had made all my experiments on air while I was there. It was such an one as is there commonly used for washing linen."

In 1772 Lord Shelburne wished for a "literary

companion," and Priestley was induced to accept the office by the offer of a good salary, a house and other appointments, together with an annuity at the end of the engagement. Fortunately for science, his lordship had scarcely any duties for his literary companion to perform, and Priestley was thus able to give most of his time to the continuation of his chemical work. He remained with Lord Shelburne seven years.

He then settled in Birmingham, and accepted the charge of a congregation which he characterises as the most liberal in England. He was now nearly sixty years of age, free from embarrassment of every kind, and happy in the friendship of such men as Boulton and Watt, the engineers; Wedgwood, the potter; Keir, Withering, Darwin, and the Galtons. He had ample leisure for his work, and no lack of encouragement and substantial help when needed. The picture of his life which he draws at this time indicates his serenity of mind and his sense of rest. He is thankful to that good Providence which always took more care of him than he ever took of himself, and he esteems it a singular happiness to have lived in an age and country in which he had been at full liberty both to investigate, and, by preaching and writing, to propagate religious truth. This calm, however, was but the presage of a great storm, and it burst over the old philosopher during the loud strife of party passion which agitated this country at the outbreak of the French Revolution. On the occasion of a public dinner on the anniversary of the taking of the Bastille, at which dinner Priestley was not present, and with which it does not appear that he had anything to do, a mob attacked and wrecked, in the name of "Church and King," the chapels and houses of the Dissenters in the town. The full fury of the rising

seemed to be concentrated upon Priestley, and he and his family barely escaped with their lives, leaving library, papers, and instruments to the tender mercies of the insane crowd, who speedily demolished what had been the labour and fruit of years. Priestley with difficulty got to London, but so uncertain was the temper of the time that his friends forcibly kept him in hiding for some weeks. His appeal for redress met with but a tardy acknowledgment, and the recompense which he eventually received was absurdly disproportionate to his disastrous experience of what Mr. Pitt was pleased to call "the effervescence of the public mind." His sons, disgusted with the justice which he received, left the country, and eventually settled in America. Although he himself was not without a position, for he was invited to minister to a large congregation at Hackney before he had been many months in London, and his friends vied with each other in rendering him help, his situation was still hazardous: his scientific brethren turned their backs upon him, his servants feared to remain with him, and the tradespeople declined to have his custom. At length he determined to follow his sons. Before he left he wrote these remarkable words: "I cannot refrain from repeating again, that I leave my native country with real regret, never expecting to find anywhere else society so suited to my disposition and habits, such friends as I have here (whose attachment has been more than a balance to all the abuse I have met with from others), and especially to replace one particular Christian friend, in whose absence I shall, for some time at least, find all the world a blank. Still less can I expect to resume my favourite pursuits with anything like the advantages I enjoy here. In leaving this country I also abandon a source of maintenance

which I can but ill bear to lose. I can, however, truly say that I leave it without any resentment or ill-will. On the contrary, I sincerely wish my countrymen all happiness; and when the time for reflection (which my absence may accelerate) shall come, they will, I am confident, do me more justice. They will be convinced that every suspicion they have been led to entertain to my disadvantage has been ill-founded, and that I have even some claim to their gratitude and esteem. In this case I shall look with satisfaction to the time when, if my life be prolonged, I may visit my friends in this country; and perhaps I may, notwithstanding my removal for the present, find a grave (as I believe is naturally the wish of every man) in the land that gave me birth." He never returned. His sons had settled at Northumberland, a little town placed in one of the most beautiful spots on the Susquehanna. Here, surrounding himself with books and taking but little interest in the politics of the country, he occupied himself to the last with philosophy and his beloved theology; steadily refusing to become naturalized, although the expediency of such a step was frequently pressed upon him, saying that "as he had been born and lived an Englishman he would die one, let what might be the consequence."

Priestley is mainly remembered by his theological controversies and his contributions to the history of pneumatic chemistry. I have nothing to tell you of his merits as a controversialist, except to say that some of his argumentative pieces are among the most forcible and best written of his literary productions. It is on his chemical work that his reputation will ultimately rest: this will continue to hand down his name when all traces of his other labours are lost. He has frequently

been styled the *Father of Pneumatic Chemistry*; and although we may question the propriety of the appellation when we call to mind the labours of Van Helmont, of Boyle, and of Hales, there is no doubt that Priestley did more to extend our knowledge of gaseous bodies than any preceding or successive investigator.

Priestley was born just as Stahl, the author of what is known in the history of chemistry as the *Phlogistic Theory*, had run out his course. To this theory, handed down as it seemed to his especial keeping, Priestley unswervingly adhered. But, by a strange perversity of fate, the very discoveries which he brought forward as the strongest proofs of the soundness of the Phlogistic doctrine have conduced, perhaps more than any other set of facts, to its destruction. Let me attempt to give you some other notion of this Phlogistic Theory. A piece of wood burns: a piece of stone does not. Why is this? "Because," answers Stahl, "the wood contains a peculiar principle—the principle of inflammability: the stone does not. Coal, charcoal, wax, oil, phosphorus, sulphur—in short, all combustible bodies—contain this principle in common: to this principle (which, indeed, I regard as a material substance) I give the name of *Phlogiston*. I regard all combustible bodies, therefore, as compounds, and one of their constituents is this phlogiston: the differences which we observe in combustible substances depend partly upon the proportion of the phlogiston they contain, and partly upon the nature of the other constituents. When a body burns it parts with its phlogiston; and all the phenomena of combustion—the heat, the light, and the flame—are due to the violent expulsion of that substance. This phlogiston lies at the basis of all chemical change: all chemical reactions are so many manifestations of parts played by

phlogiston." If zinc be strongly heated it takes fire and burns with a beautiful greenish flame, and a white or yellowish-white substance remains behind. "Phlogiston," says Stahl, "is here making its escape. Zinc is composed of phlogiston and the white earthy powder—which I term *calx* of zinc—which now becomes visible." If I melt some lead, and keep it well stirred, it gradually becomes converted into a powder, first of a yellow and ultimately of a beautiful red colour. Phlogiston has thus been gradually expelled, its expulsion having been promoted by stirring the mass, and the *calx* of lead—the other constituent of the metal—becomes evident. To remake the metal it is merely necessary to impart phlogiston to the *calx*, and any substance that will give up its phlogiston may be employed for that purpose. If the red lead or the *calx* of zinc be heated with wood or charcoal, or resin, or phosphorus, or sulphur, the respective metals will be regenerated. Too much of the phlogiston, however, will destroy the metallic nature of the lead or the zinc. If we employ an excess of phosphorus or sulphur (bodies very rich in phlogiston, as their excessive inflammability shows) the metals will combine with the superabundant phlogiston and lose their metallic character.

I told you that in heating the lead the *calx* had, to begin with, a yellow colour, and that it only became red by the prolonged action of the fire. The change in the colour affords a measure of the rate of the expulsion of the phlogiston. When in the yellow stage the *calx* has not parted with the whole of the phlogiston: as we continue to heat it more phlogiston is expelled, and the mass becomes red. So, too, if, in performing the reverse operation, we add an insufficient amount of phlogiston, the red *calx* is not converted into metal—it is only brought back to the yellow stage. In some such

manner as this the Stahlian doctrine attempted to account for the colours of substances.

We all know that if a candle is burnt in a limited amount of air the flame will shortly be extinguished, although no change apparently takes place in the air. This was explained, according to Stahl's doctrine, by supposing that air had an affinity for phlogiston, and that in the act of combustion the phlogiston was transferred from the candle to the air. Gradually, however, the limited amount of air becomes saturated with phlogiston—that is, wholly phlogisticated—and combustion accordingly ceases. In like manner, if a mouse is placed in a confined volume of air, after a time it experiences difficulty in breathing and eventually is suffocated, although the bulk of the air remains the same. The act of breathing, therefore, is nothing else than the transference of phlogiston from the animal to the air, which gradually becomes phlogisticated and is thereby unable to support respiration. To this doctrine of phlogiston, originally broached as a theory of combustion and gradually extended into a theory of chemistry, nearly every European chemist for upwards of half a century after its author's death gave an implicit adherence.

Priestley, whilst at Leeds, lived near a brewery : it was this circumstance that first directed his attention to chemical matters. He had read of *fixed air*, the gas which we now style carbon dioxide or carbonic acid ; and being desirous of making himself acquainted with its properties, he took advantage of the fermentative process in which it is abundantly formed to procure some. Priestley at this time had little or no knowledge of chemistry ; he was possessed of no apparatus, and had scarcely the means of procuring any. But these

very circumstances were the sources of his success, since he was under the necessity of devising original processes and appliances suited to his narrow means and peculiar views. "If," he says, "I had been previously accustomed to the usual chemical processes, I should not have so easily thought of any other, and without new modes of operation I should hardly have discovered anything materially new." One of the earliest pieces of apparatus which he devised is the well-known *pneumatic trough*—a simple enough piece of chemical furniture certainly, but one that required a considerable amount of experimenting with before it took its present shape. In his experiments with fixed air he observed that this gas conferred "a pleasant acidulous taste" on water, so that he was able in two or three minutes to make a "glass of exceedingly pleasant sparkling water, which could hardly be distinguished from very good Pyrmont, or rather seltzer water." He likewise observed that "the pressure of the atmosphere assists very considerably in keeping fixed air confined in water. . . . I do not doubt, therefore, but that, by the help of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring; and it would not be difficult to contrive a method of doing it." Priestley here throws out the idea of the manufacture of "soda water"—"a service," says Mr. Huxley, "to naturally, and still more to artificially, thirsty souls, which those whose parched throats and hot heads are cooled by morning draughts of that beverage, cannot too gratefully acknowledge."

Priestley was next attracted by the singular properties of hydrogen, or *inflammable air*, as it was then termed—a gas which had already been made the subject of an elaborate memoir by Mr. Cavendish. Cavendish

was inclined to suppose that inflammable air was phlogiston in the free state—an opinion contrary to the belief of Stahl and his immediate followers, who imagined that phlogiston was a solid earthy volatile substance. In order to get some clue as to the nature of this protean body, Priestley placed a quantity of minium or the *calx of lead*—that is, lead from which the phlogiston has been expelled—within a tall cylinder, filled with inflammable air, and standing over water. He then proceeded to heat the calx by means of a burning lens—a method which he constantly employed, and which materially contributed to many of his discoveries. Let us give the result in his own words: “As soon as the minium was dry, by means of the heat thrown upon it, I observed that it became black, and then ran in the form of perfect lead; at the same time that the air diminished at a great rate, the water ascending within the receiver. I viewed this process with the most eager and pleasing expectation of the result, having at that time no fixed opinion on the subject; and therefore I could not tell except by actual trial whether the air was decomposing in the process, so that some other kind of air would be left, or whether it would be absorbed *in toto*. The former I thought the more probable, as if there was any such thing as phlogiston, inflammable air, I imagined, consisted of it and something else. However, I was then satisfied that it would be in my power to determine, in a very satisfactory manner, whether the phlogiston in inflammable air had any *base* or not; and if it had, what that base was. For, seeing the metal to be actually revived, and that in a considerable quantity, at the same time that the air was diminished, I could not doubt but that the calx was actually imbibing something from the air; and

from its effects in making the calx into metal, it could be no other than that to which chemists had unanimously given the name of *phlogiston*."

This experiment he repeated with every precaution, and in every conceivable manner—varying the nature of the calx, sometimes taking the calx of tin, of bismuth, of mercury, of silver, of iron, and of copper—and sometimes making the experiment over quicksilver instead of water. He found that the inflammable air was totally absorbed; and, accordingly, he concludes—"that phlogiston is the same thing as inflammable air, and is contained in a combined state in metals, just as fixed air is contained in chalk and other calcareous substances: both being equally capable of being expelled again in the form of air."

Priestley then proceeded to determine the amount of the phlogiston which must be contained in the various metals; by ascertaining the quantity of inflammable air taken up by their calces. He found that 1 oz. of lead was revived by the absorption of 108 oz. measures of inflammable air, and 1 oz. of tin by the absorption of 377 oz. measures. Let me direct your attention for a moment to these numbers, since they afford us a ready means of determining the degree of accuracy with which Priestley made his observations. The 108 oz. measures of hydrogen required to revive the 1 oz. of lead are equivalent to 204·1 cubic inches, and weigh, at the ordinary temperature, about 4·4 grains. Now, the most refined processes of modern chemical analysis have shown that the weight of hydrogen required to regenerate 1 oz. of lead from the yellow calx is 4·6 grains—no great disparity, after all, from Priestley's result. The 377 oz. measures of hydrogen required to revive 1 oz. of tin would weigh about 15·4 grains; modern chemistry

says that the exact quantity needed is 16·3 grains. Priestley was here on the verge of a great discovery—a discovery which, in the first place, would have given a crushing blow to Stahl's doctrine—and which, in the second, might have ended in the determination of a fact of no less magnitude than the true composition of water. But his phlogistic ideas rendered him blind to the full significance of his results. He was prepossessed with the notion that by phlogisticating the calx it gained in weight, and that the weight of the metal formed must be equal to the weight of the calx *plus* that of the phlogiston absorbed. He tells us that he frequently attempted to ascertain the weight of the inflammable air in the calx, “so as to prove that it had acquired an addition of weight by being metallized,” but the result never came out in accordance with the theory. This, he satisfies himself, must be due to part of the calx subliming, and part being dissolved by the mercury; and he concludes, “that were it possible to procure a perfect calx, no part of which should be sublimed and dispersed by the heat necessary to be made use of in the process, I should not doubt but that the quantity of inflammable air imbibed by it would sufficiently add to its weight.” Every sound phlogistian for at least a quarter of a century after Stahl's death believed that when a metal was calcined the calx *must* weigh less than the metal: for had not phlogiston been expelled? There were indeed certain vague rumours that various people had found it otherwise: Boyle had made some experiments with tin; a French surgeon named Rey had experimented upon lead; and an obscure alchemist called Sulzbach had recorded some observations upon mercury; but then these people had not had the good fortune to work in the light of the phlogistic doctrine,

or they were sceptics who were justly punished for their unbelief by their false results. But about Priestley's time it gradually dawned upon the phlogistians that the sceptics and ignorant people might be right after all, for some of their own trusted number had condescended to repeat the experiments which so obstinately refused to chime in with the established order of things, and found, doubtless to their dismay, that it could no longer be gainsaid that a metal by calcination *gained* in weight. But the phlogistians were not going to see their beautiful superstructure—a theory in which all the parts seemed to fit so nicely—brought ignominiously down by the trivial weight of such a fact as this. We concede, said they, that we have been in error respecting the precise nature of phlogiston : it cannot be the gross earthy substance that Stahl had taught us to believe in. It is plainly something far more etherealised—a sort of invisible, imponderable ether—the very *principle of levity*, in fact, a principle so very light that so far from adding to the weight of bodies with which it combines, it actually makes them lighter than they were before ! It seems scarcely credible, but this was precisely the position taken up by a large section of the phlogistians ; not by all of them, however, for some were sagacious enough to see that a theory which needed a hypothesis of this character to bolster it up must be rapidly on the wane. “Of late,” writes Priestley, “it has been the opinion of many celebrated chemists, Mr. Lavoisier among others, that the whole doctrine of phlogiston is founded on mistake. The arguments in favour of this opinion, especially those which are drawn from the experiments Mr. Lavoisier made on mercury,¹ are so specious that I own I was myself much inclined to adopt

¹ A repetition of the experiments of Sulzbach.

it." And Priestley assuredly would have adopted it if he could only have looked at the results of his experiments otherwise than through the fogs of his prejudices. He would have grasped the fact that with the disappearance of ponderable inflammable air (for light as it is it could not have been the principle of levity), the calx *lost* weight, and by much more than the weight of the inflammable air. This fact once properly laid hold of might have explained the origin of that water which he distinctly noted as being produced in his trials over mercury. In one of his experiments he heated a quantity of the calx of mercury in inflammable air, and although, as he tells us, "the gas was previously well dried with fixed ammoniac," water was found in "sufficient quantity." "This experiment," he goes on to say, "may be thought to be favourable to the hypothesis of water being composed of fixed and inflammable air: as all water was carefully excluded, and yet a sufficient quantity was found in the process." But to the notion of the compound nature of water he attaches no weight. The water he supposes came either from the calx or, which he thinks more probable, from the inflammable air—that it was in fact essential to the constitution of the gas; an opinion which became a conviction when he observed how frequently water was formed in processes in which the inflammable air played a part.

When steam is driven through a red-hot iron tube inflammable air, the phlogiston of Priestley and Cavendish, is produced in abundance—a fact first observed by Lavoisier; but then, as Priestley says, "Mr. Lavoisier is well known to maintain that there is no such thing as what has been called *phlogiston*; affirming inflammable air to be nothing else but one of the elements or constituent parts of water. As to myself, I was a long

time of opinion that his conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process. But though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. Watt's observations threw upon them, satisfied me, at length, that the inflammable air came from the iron." The arrangement which Priestley made use of in these experiments is identical with that which we use on our lecture tables to-day for the same purpose. Steam is driven through an iron tube heated to redness, and the inflammable air is collected in one of Priestley's pneumatic troughs. "Of the many experiments which I made with iron," says Priestley, "I shall content myself with reciting the following results. With the addition of 267 grains to a quantity of iron, and the loss of 336 grains of water, I procured 840 ounce measures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the consumption of 254 grains of water, I got 420 ounce measures of air." These numbers again serve to test the accuracy of Priestley's work. In the first experiment the iron gained 267 grains, and the yield of inflammable air was 840 ounce measures. 840 ounce measures of hydrogen, at the ordinary temperature, weigh 34.3 grains; that is, the gain of the iron was $7\frac{3}{4}$ times the weight of the inflammable air. Assuming, then, with Lavoisier, that water is a compound, and that one constituent is fixed by the iron and the other makes its escape as inflammable air, it would follow from Priestley's experiment that water is composed of $7\frac{3}{4}$ parts by weight of the substance fixed by iron, united to 1 part by weight of inflammable air. Modern science has completely established the correctness of Lavoisier's opinion, and disproved

that of Priestley, but it has added little, even with all its elaborate processes of quantitative analysis, to the results of Priestley's trials. Water is composed of oxygen—the substance fixed by the iron—and inflammable air, or hydrogen; and the proportion by weight of the former gas to the latter is almost exactly as 7·9 to 1.

Acting upon some remarks by Mr. Cavendish, Priestley was led to study the action of *aqua fortis*, or “nitrous acid,” as it was then called, upon the metals. Trying first upon brass, and then upon copper, he obtained a gas to which he gave the name of *nitrous air*, but which is now called *nitric oxide*. “One of the most conspicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a considerable heat. . . . The diminution of a mixture of this and common air is not an equal diminution of both the kinds . . . but of one-fourth of the common air, and as much of the nitrous air as is necessary to produce that effect. . . . I hardly know any experiment that is more adapted to amaze and surprise than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is considerably diminished by it. It is exceedingly remarkable that this effervescence and diminution, occasioned by the mixture of nitrous air, is peculiar to common air, or *air fit for respiration*, and, as far as I can judge from a great number of observations, is at least very nearly, if not exactly, in proportion to its fitness for this purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by

putting mice, or any other animal, to breathe in it." Upon this principle Priestley devised a method of measuring the quality of air. A small phial, termed the *air measure*, about an ounce in capacity, was filled with the air to be examined, which was then transferred to a jar about $1\frac{1}{2}$ inches in diameter, previously filled with water. The air measure was then filled with the nitrous air and emptied into the jar containing the air to be analysed. The mixture was allowed to stand for about two minutes, and was then transferred to a glass tube about two feet long and one-third of an inch wide, graduated in terms of the air measure, and divided into tenths and hundredth parts. The volume of the residual gas was then read off, care being taken to immerse the tube to such a depth in the trough that the water in the inside and on the outside was on the same level. The result was expressed in measures and parts of a measure : thus, if on mixing equal volumes of common air and nitrous air the residual volume was one measure and two-tenths of a measure, the standard of the air was said to be 1·2.

With this instrument Priestley attempted to measure the difference between good air and that which was reputed to be unwholesome ; but, although he compared the worst air he could get from manufactories, from coalpits, and from the holds of ships, with the best country air, he was unable to perceive any difference ; and he was satisfied, therefore, " that air may be very offensive to the nostrils, probably hurtful to the lungs (and, perhaps, also in consequence of the presence of phlogistic matter in it), without the phlogiston being so far *incorporated with it* as to be discoverable by the mixture of nitrous air. . . . I have frequently taken the open air in the most exposed places in the country, at

different times of the year and in different states of the *weather*, etc., but never found the difference so great as the inaccuracy arising from the method of making the trial might easily amount to or excel." Other experimenters, less conscientious than Priestley, found the differences they sought for; but the researches of Bunsen, of Regnault, and of Dr. Angus Smith, made with all the precision of modern gasometric analysis, have shown that the atmosphere is wonderfully constant in composition, and that, although there are variations, they are infinitely beyond the cognisance of the nitrous air test.

A second observation by Mr. Cavendish led Priestley to another discovery. Cavendish, in the course of the work on inflammable air to which I have alluded, attempted to prepare that gas by acting on copper with spirit of salt, or "marine acid," as it was then commonly called. Instead of the wished-for result, he procured "a much more remarkable kind of air, viz. one that lost its elasticity by coming in contact with water." By substituting quicksilver for water in his trough, Priestley obtained this air in quantity, and examined its properties. He quickly found that the copper played no part in the process of making the gas, for on heating the acid alone he procured it just as readily. "So that," he says, "this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of spirit of salt, which appear to be of such a nature that they are not liable to be condensed by cold, like the vapour of water and other fluids; and therefore may be very properly called an *acid air*, or more restrictively, the *marine acid air*." Spirit of salt, or, as chemists also term it, hydrochloric acid, is therefore nothing else than a solution of Priestley's marine acid air in water.

This discovery induced Priestley to try the same experiment with other acids, and, among them, with oil of vitriol. But he says, "I got no air from the oil of vitriol by any application of heat. But in attempting to procure it, I got it by means of *mercury* in a manner that I little expected, and I paid pretty dearly for the discovery it occasioned. Despairing to get any air from the longer application of my candles, I withdrew them ; but before I could disengage the phial from the vessel of quicksilver, a little of it passed through the tube into the hot acid, when instantly it was all filled with dense white fumes, a prodigious quantity of air was generated, the tube through which it was transmitted was broken into many pieces (I suppose by the heat that was suddenly produced), and part of the hot acid being spilled upon my hand burned it terribly, so that the effect of it is visible to this day. The inside of the phial was coated with a white saline substance, and the smell that issued from it was extremely suffocating. . . . Not discouraged by the disagreeable accident above mentioned, the next day I put a little quicksilver into the phial along with the oil of vitriol, when, before it was boiling hot, air issued plentifully from it." The new gas with which Priestley was rewarded for his pain and perseverance he termed *vitriolic acid air* : it is now known as sulphur dioxide, and is precisely the same substance which is produced on burning brimstone in the air. You have doubtless all noticed its formation on striking an old-fashioned lucifer match.

I daresay many of you have seen the beautiful etchings made upon glass by means of *hydrofluoric acid*—an acid first obtained by a contemporary of Priestley, named Scheele—a poor Swedish apothecary, and one of the greatest chemists of the 18th century. Glass, as you

are doubtless aware, is a mixture of sand or silica, lime, alkali, and occasionally red lead. The hydrofluoric acid acts upon the glass by seizing upon the silica and forming with it a gaseous substance termed by chemists *fluoride of silicon*. This fluoride of silicon was obtained by Priestley by heating a mixture of fluor spar, or Derbyshire spar, with oil of vitriol in a glass vessel. When this gas (which he termed *fluor acid air*) is led into water it is instantly decomposed, and silica is reproduced. The formation of this silica constitutes a very striking experiment; so much so, that, says Priestley, "I have met with few persons who are soon weary of looking at it, and some could sit by it almost a whole hour and be agreeably amused all the time."

I doubt not that you are all familiar with that pungent, tear-exciting liquid termed by the apothecaries "spirits of hartshorn," or ammonia. This substance has been known for a very long time: its name, "ammonia," is derived from the circumstance that it was prepared, ages ago, by the Arabs in the desert near the temple of Jupiter Ammon. Now, although this liquid has been known for some thousands of years, it required Priestley to tell us that its peculiar properties were due to a gas held in solution. Priestley treated the spirit of hartshorn as he had treated the spirit of salt, and he presently found that a great quantity of a transparent and, apparently, permanent air was discharged from it. He ascertained all the more striking attributes of this "alkaline air," as he termed it; among others, its solubility in water and its inflammability. He next proceeded to determine its composition by passing electric sparks through it, and he found that, after passing the sparks until no further increase of bulk could be observed, the gas was ultimately trebled in

volume, and that no part of it was soluble in water. The gas, in fact, had been decomposed into its constituents—into hydrogen (the presence of which Priestley recognised), and into nitrogen, which he calls *phlogisticated air*, and which, he says, is contained to the extent of one-fourth of the bulk of the mixture. He then tried the action of the alkaline air upon the airs which he had previously discovered, and notably upon the “marine acid air,” as he had “a notion that these two airs, being of opposite natures, might compose a *neutral air*, and perhaps the very same thing with common air. But the moment that these two kinds of air came into contact a beautiful white cloud was formed, and there appeared to be formed a solid *white salt*, which was found to be the common *sal ammoniac*, or the marine acid united to the volatile alkali.”

If by some evil chance the cold and damp of this coming winter should drive some of you to the dentist, and if after seating you in that awful chair and harrowing your distracted nerves with the sight of his murderous tools, he humanely offers to send you to sleep with his nitrous oxide, by all means let him, and, when you wake with the sweet consciousness that “it is all over,” give a passing benediction to the memory of Priestley, for he first told us of the existence of that gas.

If, too, as you draw up to the fire “betwixt the gloaming and the mirk” of these dull, cold November days, and note the little blue flame playing round the red-hot coals, think kindly of Priestley, for he first told us of the nature of that flame when in the exile to which our forefathers drove him.

The crowning work of Priestley’s life was, however, the discovery of that gas which he termed *dephlogisticated air*, but to which Lavoisier, who swept away all

the jargon of the Phlogistic doctrine, gave the name of Oxygen. The manner of this discovery is characteristic of much of Priestley's work. "It furnishes," he says, "a striking illustration of the truth of a remark which I have more than once made in my philosophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philosophical investigations; viz. that more is owing to what we call *chance*, that is, philosophically speaking, to the observation of *events arising from unknown causes*, than to any proper *design* or preconceived *theory* in this business." The accident of possessing a burning glass "of considerable force" led Priestley to try the effect of the heat of the sun upon various substances contained in tubes filled with mercury, and standing over the mercurial trough. "With this apparatus, after a variety of other experiments, an account of which will be found in its proper place, on the 1st of August 1774 I endeavoured to extract air from *mercurius calcinatus per se*—that is, calx of mercury, and I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can well express was, that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous gas exposed to iron or liver of sulphur [that is, his nitrous oxide gas]; but as I had got nothing like this remarkable appearance from any kind of air besides this particular modification of nitrous air, and I knew no nitrous air was used in the preparation of *mercurius calcinatus*, I was utterly at a loss how to account for it." His astonishment was still further increased when

he found that, tested with his nitrous air, the new gas was actually better than common air, and that mice would live longer in it than in an equal bulk of that air. He had the curiosity to breathe it himself. "The feeling of it to my lungs was not sensibly different from that of common air; but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that in time this pure air may become a fashionable article in luxury? Hitherto only two mice and myself have had the privilege of breathing it. . . . But, perhaps, we may also infer from these experiments, that though pure dephlogisticated air might be very useful as a *medicine*, it might not be so proper for us in the usual healthy state of the body; for, as a candle burns out much faster in dephlogisticated than in common air, so we might, as may be said, *live out too fast*, and the animal powers be too soon exhausted in this pure kind of air. A moralist, at least, may say, that the air which nature has provided for us is as good as we deserve."

Priestley at length got to the conclusion that common air was no longer a "*simple elementary substance*, indestructible and unalterable," but that it was composed of 1 volume of his new air and 4 volumes of phlogisticated air. This new air, he concluded, was devoid of phlogiston—hence the term "dephlogisticated air," but that in the processes of respiration and combustion phlogiston was imparted to it. Priestley found that he could obtain this air from the calx of lead as well as from the calx of mercury, and this fact, he says, "confirmed me more in my suspicion that the *mercurius calcinatus* must have got the property of yielding this kind of air from the atmosphere, the process by which that preparation, and this of red lead, is made being similar. As I never make the least secret of anything

that I observe, I mentioned this experiment also, as well as those with the *mercurius calcinatus*, to all my philosophical acquaintances at Paris and elsewhere, having no idea at that time to what these remarkable facts would lead." The knowledge which Priestley, as he tells us, imparted to the French chemists was used by them with crushing effect against his favourite theory. The discovery of oxygen was the deathblow to phlogiston. Here was the thing which had been groped for for years, and which many men had even stumbled over in the searching, but had never grasped. Priestley indeed grasped it, but he failed to see the magnitude and true importance of what he had found. It was far otherwise with Lavoisier. He at once recognised in Priestley's new air the one fact needed to complete the overthrow of Stahl's doctrine; and now every stronghold of phlogistonism was in turn made to yield. Priestley, however, never surrendered, even when nearly every phlogistian but he had given up the fight or gone over to the enemy. When age compelled him to leave his laboratory he continued to serve the old cause in his study, and almost his last publication was his *Doctrine of Phlogiston Established*. His own life, indeed, affords an exemplification of the truth of his own words, that "we may take a maxim so strongly for granted, that the plainest evidence of sense will not entirely change, and often hardly modify, our persuasions; and the more ingenious a man is, the more effectually he is entangled in his errors, his ingenuity only helping him to deceive himself by evading the force of truth."

III

CARL WILHELM SCHEELE

AN ADDRESS TO THE OWENS COLLEGE CHEMICAL SOCIETY, AT THE
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IN the personal history of learning there are few more striking or, in a sense, more romantic figures than the chemist Scheele. "La vie de M. Scheele," wrote Vicq d'Azyr, "offre l'exemple d'un savant modeste qui, dédaignant tout éclat, eut le courage de vivre obscur ; dont le zèle n'eut pas besoin d'être excité par la louange, et qui, connu des gens de l'art, mais presque ignoré de son siècle, avoit rendu son nom immortel lorsqu'il n'avoit pas encore de célébrité."¹ An obscure apothecary, living a solitary sedentary life in a small town on the shore of a Scandinavian lake, hampered by poverty and harassed by debt, hypochondriacal, and, at times, the victim of the most depressing melancholy—he yet succeeded by the sheer force of his genius as an experimentalist, and under the influence of a passion which defied difficulty and triumphed over despair, in changing the entire aspect of a science. No man ever served chemistry more loyally or with a purer, nobler, more disinterested devotion than Scheele. "Diese edel Wissenschaft," he wrote to his friend Gahn, "ist mien Auge."

¹ *Éloges historiques*, vol. ii. p. 19.

The pursuit of truth for its own sake—with no thought of worldly gain or reward—was to him the supreme object of his existence and the highest form of his religion. The cause of science was, indeed, as sacred to him as if it were that of a martyr, and he gave up his life to her service with a martyr's spirit of patience, self-sacrifice, and humility.

But although Scheele's name is associated with some of the most remarkable discoveries of the eighteenth century, and of which the value was quickly recognised by his contemporaries, comparatively little is known of his personal characteristics, of his habits of work, or of the nature of his surroundings. Practically the only mental picture of him that we have hitherto been able to form is to be derived from the memorial notice of him by Sjösten, the Secretary of the Stockholm Academy of Sciences, which appears in the *Proceedings* of the Academy for 1799, that is thirteen years after Scheele's death. Sjösten was not a chemist, and was otherwise unfitted to judge of the merit and true proportion of Scheele's work. He appears to have obtained his information from materials collected by his predecessor in office, Johan Carl Wilcke, whose name is honourably known in the history of science from his connection with the discovery of latent heat. On the death of Scheele, Wilcke placed his papers and laboratory notes in the charge of the Academy, which subsequently came into possession of Scheele's correspondence with Retzius, Gahn, and Hjelm. From this rich material, together with a collection of letters to Bergmann, preserved in the University of Upsala, Wilcke conceived the idea of preparing an account of Scheele's life and labours which should set forth the origin and chronological history of his investigations, and so exhibit his true relations as

a discoverer to his great contemporaries, Cavendish, Priestley, and Lavoisier. Unfortunately the realisation of this project was frustrated by Wilcke's death. Thanks, however, to the piety and patriotism of Baron Nordenskiöld this valuable collection of letters and laboratory memoranda has now been given to the world, and the historian of chemistry is at length in a position to determine much in Scheele's life that has hitherto been doubtful and obscure.¹

M. Nordenskiöld has been materially aided in his work by the *Lars Hiertas minne* Trust, and, above all, by the zeal of Mme. Elin Bergsten, who undertook not only to transcribe the letters, which are difficult to read on account of their archaic style and antiquated language and the constant employment in them of an obsolete nomenclature, but also to decipher the laboratory notes, which are for the most part rough jottings of experimental results put together by means of contractions and a system of symbols wellnigh as illegible as that of the alchemists.

The handsome well-printed volume which embodies the results of so much patient and conscientious labour has appeared at a timely moment; indeed, no more fitting memorial of the one hundred and fiftieth anniversary of the birth of the great Swedish chemist could be conceived than the publication of a work which fixes for all time, without question or cavil, his true relation to his epoch, and his place in the history of scientific discovery. Scheele, who took little thought for his own fame, owes much to women; for, it is worth noting, Mme. Bergsten is not the first of her sex who has striven to perpetuate his genius. It was through Mme. Picardet,

¹ *Carl Wilhelm Scheele: Nachgelassene Briefe und Aufzeichnungen.* Herausgegeben von A. E. Nordenskiöld. Stockholm: Verlag von P. A. Norstedt & Söner.

the wife of a magistrate at Dijon, that France first gained a knowledge of his memoirs. Instigated by De Morveau, she learned German and Swedish solely for the purpose of translating Scheele's papers.

Carl Wilhelm Scheele was born on 9th December 1742 at Stralsund, at that time the capital of Swedish Pomerania. He was the seventh child in a family of eleven. His father, Joachim Christian Scheele, was a merchant of some note in Stralsund. He came of a good stock, branches of which had occupied important positions in North Germany as far back as the fifteenth and sixteenth centuries. One member became Bishop of Lübeck, and another distinguished himself as an admiral in the Swedish service in the time of Charles XI. A female connection of the family, Anna Scheele, was the mother of Wilcke, the Secretary of the Swedish Academy of Sciences, whose name has already been mentioned as having projected a biography of his illustrious relative. The Stralsund merchant was apparently not in a position to afford his sons the advantages of a university training. Carl Wilhelm was placed at a private school in his native town, and after having acquired a fair knowledge of Latin he passed on to the gymnasium. He seems to have been a thoughtful, studious boy, remarkable among his fellows for diligence and for the ease and rapidity with which he accomplished his school tasks. The bent of his mind towards science would appear to have manifested itself even at this time; at all events, he then acquired that facility in writing chemical symbols which characterised his letters and memoranda, and the apothecary Cornelius, who gave him instruction in reading pharmaceutical receipts and prescriptions, has testified to his aptitude for chemical study and speculation. It is not improbable,

however, that the course of his inclination may have been, to some extent, directed from home. His eldest brother, Johann Martin, had been apprenticed to an apothecary in Gothenburg named Bauch, but had died whilst Carl Wilhelm was at school. Three years afterwards, that is when fourteen years of age, he too was apprenticed to Bauch. The Gothenburg apothecary seems to have been an honest, even-handed man, who, to judge from the inventory of his possessions in the archives of the Rathhaus of the town, followed his calling in a worthy, liberal-minded fashion. In Bauch's laboratory Scheele made the practical acquaintance of nearly all the pharmaceutical and chemical products of his time. He had also access to such standard works as Neumann's *Praelectiones Chemicæ*, Lémery's *Cours de Chimie*, Boerhaave's *Elementa Chemicæ*, Kunckel's *Laboratorium Chymicum*, and Rothe's *Anleitung zur Chymie*. Nor was he slow to avail himself of his opportunities. Bauch, in letters to the Stralsund home, fears for the health of his young charge, who devotes hours which should be given to sleep either to the study of books which are beyond his years, or to the making of experiments that would tax the skill of his older fellow-apprentices. Kunckel's *Laboratorium* and Neumann's *Chymie* seem, indeed, to have been his chief instructors in practical chemistry, and it was by diligently repeating the experiments contained in these books that he laid the foundations of the manipulative skill and analytical dexterity on which his success as an investigator ultimately rested.

In 1765 Bauch, then an old man, sold his business, and Scheele, now twenty-three years of age, took service with Kjellström, an apothecary in Malmö, with whom he remained about a couple of years.

Kjellström has recorded his opinion of his young assistant, but it is from his fellow-worker and friend Retzius that we derive the most vivid conception of Scheele at this period of his career. Anders Johan Retzius was of the same age as Scheele, and, like him, began life as a pharmacist. Eventually he attached himself to the University of Lund, as director of its Museum and Botanical Garden, and died at Stockholm in 1821, the last survivor of the Phlogistic School of Chemists. In a communication found amongst Wilcke's papers Retzius thus records his impressions of Scheele :

His genius was wholly concerned with physical science. He had absolutely no interest in any other. . . . Although possessing an excellent memory, it seemed only fitted to retain matters relating to chemistry.

“One science only will one genius fit,” says Pope.

During his stay at Malmö he bought as many books as his small pay enabled him to procure. These he would read once or twice through, when he would remember all that he desired to recall, and never again consulted them. Without systematic training and with no inclination to generalise, he occupied himself mainly with experiments. During the time of his apprenticeship at Gothenburg he worked without plan and for no other purpose than to note phenomena ; these he could remember perfectly. Eleven years' continuous exercise in the art of experimenting had enabled him to collect such a store of facts that few could compare with him in this respect. In addition he gained a readiness in devising and executing experiments such as is rarely seen. He made all kinds of experiments without reference to any system or prearranged plan. He was thus enabled to learn what no doctrinaire could possibly acquire, since working by no formulated principles he observed much and discovered much that the doctrinaire would consider impossible, inasmuch as it was opposed to his theories. I once persuaded him during his stay at Malmö to keep a journal of his experiments, and, on seeing it, I was amazed, not only at the great number he made, but also at his extraordinary aptitude for the art.

In 1768 Scheele removed to Stockholm, where he superintended the shop of an apothecary named Scharenberg. Here his opportunities for experimenting were considerably restricted. However, a window with a sunny aspect close to his place of work enabled him to make the novel and important observation that different parts of the solar spectrum influence the decomposition of silver chloride in very different degrees. It was about this time that his name first appears in chemical literature as a discoverer. With his friend Retzius he undertook the examination of cream of tartar, and succeeded in isolating, for the first time, its characteristic acid, the properties of which he carefully studied, and from which he was enabled to conclude that it differed from all acids up to that time known.

This, however, was not the first attempt made by Scheele to contribute to the literature of science. Retzius tells us that he had forwarded to the Academy an account of an inquiry into the nature of the so-called *Globuli martiales*, a pharmaceutical preparation made by boiling finely-divided iron with a solution of cream of tartar. The paper was, for the most part, a description of experiments; it was unmethodically put together, and was without definite theoretical result. It was referred by the Academy to Bergmann, and as his opinion was adverse, it was never published, and was ultimately lost. From Scheele's correspondence with Gahn, and from the laboratory memoranda which have now been published, we are able to glean an idea of the contents of this memoir. Some of the observations were unquestionably new and not without importance. Thus Scheele found that hydrogen was evolved by the contact of organic acids with iron, and he describes an apparatus by which this gas may be obtained by the action of

water alone on iron filings. The theoretical value of these facts will be obvious from the circumstance that Cavendish, at that time the recognised authority on hydrogen, or inflammable air, as it was then termed, had stated in his classical papers on "Factitious Air," published in the *Philosophical Transactions* for 1766: "I know of only three metallic substances, namely, zinc, iron, and tin, that generate inflammable air by solution in acids, and those only by solution in the diluted vitriolic acid, or spirit of salt."

Nor was Scheele more fortunate with his second contribution — "Chemical Experiments with Sal-acetosellae" [acid potassium oxalate], which he sent to the Academy in 1768. The paper was read, but was not published—again through the intervention of Bergmann. It is doubtful if Bergmann at this time had any personal knowledge of Scheele; at all events, it is impossible to suppose that he was in any way influenced by animosity. The "hochedelgeborner Herr Professor" to whom Scheele a year or two afterwards subscribed himself as his "dienstschuldigster Knecht," and with whom he was to live in the closest bonds of sympathy and mutual esteem, although one of the most cultivated men of his age, and distinguished by the breadth of his knowledge, which ranged over zoological, physical, and cosmographical science, had at this period little acquaintance with experimental chemistry. It is hardly to be wondered at, therefore, that the crude essays of the unknown apothecary's assistant, who, like Addison's clubfellow, was somewhat awkward at putting his talents within the observation of such as should take notice of them, should have failed to commend themselves to the critical judgment and refined taste of the *homo multarum literarum*,

noted for the grace and polish of his style. There is reason to believe that these disappointments reacted upon the sensitive nature of Scheele, and that the rejection of his papers by the Academy, together with the uncongenial nature of his position in Stockholm, induced him to leave the capital in order to accept employment as a laborant in the pharmacy of Lokk at Upsala. Whatever may have been the real grounds for the change, there is no question that it was attended with the most beneficial results on Scheele's fortunes. To begin with, he was brought into personal contact with Bergmann. This *rapprochement* was due to Gahn, who had made Scheele's acquaintance in Stockholm, and who had been greatly impressed with the power and capacity of the young apothecary. It is said that Bergmann, unable to explain the change that nitre experiences when it is strongly heated, whereby it is converted into the deliquescent potassium nitrite, and evolves a ruddy gas when treated with oil of vitriol, was led by Gahn to consult Scheele, to whom the phenomena and their cause were well known. According to Retzius, the properties of the so-called *Salpeterluft*, as the ruddy gas came to be termed, were ascertained by Scheele when at Malmö, and were known to him long before anything had been written on the subject. This meeting laid the foundation of a warm and active friendship which ended only with Bergmann's death—a friendship, too, which was of the greatest service to science. "It would be difficult to say," wrote Retzius, "which of the two, Scheele or Bergmann, was the teacher or the taught. Bergmann, without a doubt, received the greater part of his practical instruction from Scheele, whilst Scheele owed to Bergmann the wider knowledge of his later years." It was at Bergmann's instigation

that Scheele undertook the epoch-making investigation of *magnesia nigra*, the *Braunstein* or pyrolusite of the German mineralogist, the "wad" of the English miner, whereby he not only showed that this substance contained a metal hitherto unknown, but also incidentally discovered oxygen, chlorine, and baryta. It may seem remarkable that Scheele, with his tastes and aptitudes, should not have followed the example of his friend Retzius, and have abandoned pharmacy for an academic career. M. Nordenskiöld finds an explanation in the assumption that the *Zunftgeist* of the time would not permit of the introduction of the *studiosus pharmaciae* within the academic circle. It is doubtful, however, whether Scheele was at all fitted, either by temperament or training, for an academic career, and as schools of chemistry were at that time constituted it is certain that he would have gained little by the change. Chemical laboratories were seldom to be found at the universities, even at the largest, and the chemical prelections of the period were, for the most part, dull and formal disquisitions unenlivened by a single experimental illustration. On the other hand, the pharmacist at that time had a right to the appellation which, in this country at least, he now too frequently usurps. He was a practical chemist in the real sense of the term, and his laboratory was of more importance to him than his shop.

Whilst with Lökk, Scheele seems to have had abundant opportunity for the prosecution of his inquiries. It was at Upsala that he collected the greater part of the experimental material for his great work on *Air and Fire*. The correspondence and laboratory memoranda which M. Nordenskiöld has given to the world, show that prior to 1773, that is at least a year before the date of

Priestley's discovery, Scheele had prepared oxygen from the carbonates of silver and mercury, from mercuric oxide, nitre and magnesium nitrate, and by the distillation of a mixture of manganese oxide and arsenic acid. It was at Upsala, too, that he began and finished his work on manganese, chlorine, and baryta; he also demonstrated the acidic character of silica and the chemical nature of magnesia, microcosmic salt, and oxalic acid.

On 4th February 1775, when thirty-two years of age, Scheele was made a member of the Swedish Academy of Sciences, a distinction never accorded, either before or since, to a student of pharmacy. In the following year he was appointed, by the Medical College, *provisor* of the pharmacy at Köping, a small town on the north shore of Lake Mälär, as successor to Hinrich Pohl, whose privilege, in conformity with Swedish law, had passed, on his death, to his young widow, Sara Margaretha Sonneman. Scheele now seemed to himself to have reached the goal of his aspirations; he had at length, he thought, obtained an independent position with the prospect of a fairly lucrative business, and he would now be able to follow his cherished projects under conditions of comparative ease and comfort. "Oh, how happy I am," he wrote to Gahn, "with never a care about eating or drinking or dwelling!" The quiet peaceful life he saw before him was to be consecrated to science. "There is no delight," he wrote, "like that which springs from a discovery; it is a joy that gladdens the heart."

But the haven of rest was not yet won. The young academician, rich in honour, was poor in means, and unlooked-for difficulties arose respecting the transfer of the lease. The widow and her father were exacting,

and other *provisors* came forward who understood the art of money-getting better than Scheele. Scheele's letters seldom contain allusions to his private affairs, but the half-dozen lines in which he makes mention to Gahn and to Bergmann of his disappointment show how deeply he felt it. Offers of assistance came from all sides. Gahn invited him to Fahlun; Bergmann wished him to return to Upsala: "Es fällt uns beiden schwer uns von einander zu trennen," he had written at the prospect of the change to Köping. The suggestion was publicly made that he should be "*chemicus regius*" in the capital. He had even invitations from abroad. D'Alembert, in a letter to Frederick II., suggested that he should be called to Berlin. "J'ai appris," he wrote, "il y a peu de temps qu'il y avait à Stockholm un très habile chimiste, nommé M. Scheele, Membre de l'Académie des Sciences de cette ville, et qui, sans m'être d'ailleurs connu, me paroît fort estimé par les plus habiles chimistes de la France." Among Wileke's papers was found a letter from the brother, Fr. Christian Scheele, from which it appears that Scheele actually did receive an invitation to Berlin with a salary of 1200 reich-thalers. Crell, the editor of the well-known *Neue Entdeckungen* and *Annalen* in which many of Scheele's papers first appeared, stated that inducements were even held out to him by the English ministry. It is difficult to know upon what basis this statement rests. Thomson, the author of the *History of Chemistry*, in mentioning the circumstance expresses his doubts as to its truth, and states that he made inquiries of Sir Joseph Banks, Cavendish, and Kirwan, but none of them had ever heard of the matter. Indeed, it is intrinsically improbable. "I am utterly at a loss," says Thomson, "to conceive what one individual in any of the ministries

of George III. was either acquainted with the science of chemistry or at all interested in its progress. . . . If any such project ever existed, it must have been an idea which struck some man of science that such a proposal to a man of Scheele's eminence would redound to the credit of the country. But that such a project should have been broached by a British ministry, or by any man of great political influence, is an opinion that no person would adopt who has paid any attention to the history of Great Britain since the Revolution to the present time." However this may be, there is one name that suggests itself as the possible author of such a project, and that is Lord Shelburne. Had Thomson been able to question Priestley on the subject, the real ground for Crell's statement might have been elicited.

But Scheele's love of quiet and retirement was too deep-seated to allow him to exchange Köping for a foreign capital. Even if he should be forced to leave the little town, Lökk was ready to take him back to Upsala. His yearning for independence and for the tranquil life which Köping had seemed to promise held him there. "One needs not to eat more than enough," he wrote to Bergmann, "and if I can find my bread in Köping, there is no occasion to seek it elsewhere." Other influences, too, were at work. The burghers of the place and the gentry of the neighbourhood combined to induce him to remain. The former, mindful, as they said, of the reproach that in parting with Scheele they would be neglectful of the benefit, no less than of the honour, to the town, declared their intention of dealing with no other apothecary; whilst the latter, headed by the principal man of the province, expressed their willingness to move for a new privilege, so as to enable him to start an independent business. This remarkable exhibition of

popular sympathy at length compelled the Sonnemans to accept the young *provisor*, and Scheele was duly installed at Köping. But herein fortune showed herself even less kind than is her wont. Scheele, after all, had gathered Dead Sea fruit. Instead of the prosperous, well-ordered business he had been led to expect, he found little but debts and discomfort. Such a blow would have crushed a weaker man. He accepted his lot uncomplainingly; we search in vain amongst the letters for a word of railing or accusation. Scheele, in truth, had been schooled in adversity, and many a hard and bitter lesson had taught him how to grapple with it. Patiently, and with a tenacity of purpose which is well-nigh sublime in its heroic self-abnegation, he deliberately set himself to retrieve the fallen fortunes of the widow's estate.

For years his life was a continual struggle with privation, relieved to some extent by an annual grant of 100 rix-thalers, which the Academy, at Bergmann's instigation, made him in 1777. In the previous year he acquired full possession of the pharmacy, and the last of the widow's debts was at length paid. The tide had now turned. In 1782 his circumstances had so far improved that he was able to build himself a new house, with a good and well-furnished laboratory. If not rich he had at least a sufficiency; a modest competency was, indeed, all he desired, for Scheele was one of those men whose riches consist, not in the abundance of their possessions, but in the fewness of their personal wants. He was now in the prime of life, and in the full maturity of his mental vigour. His scientific position was assured, and his name was mentioned with honour and respect in every intellectual centre in Europe. Many years of scientific activity were, in all human probability, before him.

Although never of robust health, he had been fairly free from illness up to his thirty-fifth year, when he contracted rheumatism from working, in the rigour of a Scandinavian winter, in the outhouse which at that time did duty as his laboratory. During the autumn of 1785 he suffered greatly, not only from rheumatism, "the natural heritage," he says, "of all apothecaries," but from a weariness and dejection even harder to bear. He still worked on, however. In the early part of 1786 he sent a memoir to the Academy on gallic acid. In the March of the same year he was studying the action of light on nitric acid. "I will repeat the experiments," he wrote, "during the coming summer. We shall then see what will come of them." That summer never came to Scheele. The rheumatism brought other disorders in its train, and he instinctively felt that his end was near. Some time before his fatal illness he had formed the resolution of marrying the widow Pohl, who, together with his sister, who died in 1780, had kept house for him at Köping. On his deathbed he carried out this project, that he might leave to her once more the business he had striven so manfully to preserve. Two days afterwards—21st May 1786—he died, in the forty-third year of his age. The brave man who had struggled with such unflinching courage in the storms of fate had conquered but to die. A new *provisor* quickly appeared, and within a few months the widow was again a wife.

The true history of Scheele's life is, after all, to be found in his works. "What we call a genius," said Pope, "is hard to be distinguished by a man himself from a strong inclination." Scheele himself would have been the first to admit that his strongest inclination was to experiment, and the rest of the world has said that herein lay his genius. His old master, Kjellström, has

recorded that such phrases as "Das kann sein"; "Das ist nicht richtig"; "Das werde ich untersuchen," were ever on his lips as he pored over the chemical literature of his time. This incessant mental activity was fruitful in investigations in every department of chemistry. We owe to Scheele our first knowledge of chlorine and of the individuality of manganese and baryta. He was an independent discoverer of oxygen, ammonia, and hydrochloric acid gas. He discovered also hydrofluoric, nitro-sulphonic, molybdic, tungstic, and arsenic acids among the inorganic acids; and lactic, gallic, pyrogallie, oxalic, citric, tartaric, malic, mucic, and uric among the organic acids. He isolated glycerin and milk-sugar; determined the nature of microcosmic salt, borax, and Prussian blue, and prepared hydrocyanic acid. He demonstrated that plumbago is nothing but carbon associated with more or less iron, and that the black powder left on solution of cast iron in mineral acids is essentially the same substance. He ascertained the chemical nature of sulphuretted hydrogen, discovered arsenetted hydrogen, and the green arsenical pigment which is associated with his name. He invented new processes for preparing ether, powder of algaroth, phosphorus, calomel, and *magnesia alba*. His services to quantitative chemistry included the discovery of ferrous ammonium sulphate, and of the methods still in use for the analytical separation of iron and manganese, and for the decomposition of mineral silicates by fusion with alkaline carbonates.

To Scheele, however, the greatest work of his life was his memoir on *Air and Fire*, which appeared in 1777, and which, on account of its relations to the chemical theory of that time, attracted universal attention, and was translated into almost every European

language. The chief part of the experimental material for this work, as is proved by the correspondence and laboratory memoranda now published, was collected partly in Malmö and Stockholm—that is, before the autumn of 1770, and partly during the earlier portion of his stay in Upsala—that is, prior to 1773. These dates are important in view of Scheele's relations as a discoverer to Priestley and Lavoisier. A number of circumstances, and more especially the dilatoriness of the publisher Swederus, retarded the appearance of the book. From the letters to Gahn it appears that the manuscript was sent to the printer towards the close of 1775, but nearly two years elapsed before the work was made public. Scheele, in several of his letters, complains bitterly of the delay. In August 1776 he wrote to Bergmann: "I have thought for some time back, and I am now more than ever convinced, that the greater number of my laborious experiments on fire will be repeated, possibly in a somewhat different manner, by others, and that their work will be published sooner than my own, which is concerned also with air. It will then be said that my experiments are taken, it may be in a slightly altered form, from their writings. I have to thank Swederus for all this." No imputation of plagiarism was ever brought against Scheele. The whole conduct of his life was proof indeed against even a suspicion of unfair dealing. Although on occasions he could show that he had the *mens sibi conscia recti*, and could manifest a proper assurance in his own vindication, he was singularly unselfish and unworldly. With all Priestley's candour and sense of rectitude, he had Cavendish's indifference to fame and his contempt for notoriety. It can hardly be doubted, however, that had Scheele's work appeared in 1775 he himself would

have occupied a still higher position in the estimation of his contemporaries, and that it would not have been left to posterity to assign him his true place in the history of scientific discovery.

It is impossible to read this, or indeed any other of Scheele's memoirs, without being impressed by his extraordinary insight, which at times amounted almost to divination, and by the way in which he instinctively seizes on what is essential and steers his way among the rocks and shoals of contradictory and conflicting observations. No man was more staunchly loyal to the facts of his experiments, however strongly these might tell against an antecedent or congenial hypothesis. "Es ist ja nur die Wahrheit," he wrote to Hjelm, "welche wir wissen wollen, und welch ein herrliches Gefühl ist es nicht, sie erforscht zu haben." Had these facts been worked out by their discoverer in the spirit of quantitative accuracy so characteristic of his contemporary Cavendish, they would inevitably have undermined phlogistonism, even if they would not have effected its overthrow, before the advent of Lavoisier. As it was, other heads and other hands made use of them to demolish the theory by which their author could alone explain them, and to which he vainly imagined they lent so strong a support. It is, perhaps, idle to speculate on the causes which prevented Scheele from recognising the full significance of his work. It may be that from the lack of mathematical training the quantitative aspects of chemistry had few attractions for him, but it is equally probable that the peculiar character of his inquiries may have been determined by the circumstances of his position, by his poverty, and by the want of the refined and costly apparatus needed for quantitative research. But surmises, as Scheele himself said, cannot

determine anything with certainty. It must be admitted that he was wanting in the faculty of co-ordination, grasp of principle, and power of generalisation, that so strikingly characterise Lavoisier; and his greatest investigation, whilst it testifies to his genius as an experimentalist, reveals, no less clearly, his weakness as a theorist. But when every legitimate deduction has been made, Scheele's work, with all its shortcomings and limitations, stamps him as the greatest chemical discoverer of his age. His story constitutes, indeed, one of the most striking examples of what may be achieved by the diligent cultivation of a single natural gift.

IV

HENRY CAVENDISH

A LECTURE DELIVERED IN THE HULME TOWN HALL, MANCHESTER, ON
24TH NOVEMBER 1875. MANCHESTER SCIENCE LECTURES

WHEN I had the honour to appear here on a former occasion I gave you some account of the life and labours of a famous Yorkshire philosopher, Joseph Priestley, one of the most illustrious of that remarkable band of learned men which did so much to make the reign of George III. what Lord Brougham was wont to declare it to be—the Augustan age of modern history.

To-night I shall venture to offer you a brief notice of the character and work of another and equally illustrious member of that band—Henry Cavendish. These two men had, however, little in common beyond their zeal for science ; indeed, it is scarcely possible to conceive of a stronger contrast than that which their personal histories afford. Priestley, the son of a poor cloth-dresser, was ardent, impulsive, ingenuous—fond of the strife of words, never so happy, indeed, as when, Ishmael-like, his hand was against everybody and everybody's hand was against him. Cavendish, a scion of a great house, was cold, retiring, reticent, passively selfish, a confirmed misogynist, a hater of noise and bustle. It was said of him that he probably uttered fewer words

in the course of his fourscore years than any man who ever lived so long—not even excepting the monks of La Trappe. Priestley delighted in literary composition; his pen was ever busy; he published more than a hundred works on subjects of the most extraordinary diversity, turning them off with an ease and rapidity which even the most prolific of lady novelists might envy. Cavendish, although he wrote much, printed fewer pages than Priestley did books; his morbid shyness, and his horror of publicity, compelled him to keep back his scientific memoirs even when he had prepared them for publication.

But that you may the better frame for yourselves some conception of the manner of man Cavendish was, let me attempt to sketch for you a scene in which he might have played a part. That there is nothing opposed to truth in it you may readily determine for yourselves, if what I say to-night may so far interest you in Cavendish as to lead you to read his life as written by Dr. Wilson or by Lord Brougham. Imagine, then, you are in the London of ninety years ago: it is night, and you are standing before an old-fashioned house in what is now a very unfashionable square. It is evident from the lights in the windows and the bustle before the door that there is a dinner-party or some social meeting in the house. A couple of chairmen have deposited a portly gentleman, with a large frill, on the step, and two or three lumbering vehicles, having set down their charges, are rattling away over the rough stones into the obscurity of the dimly-lighted street. My knowledge of London ninety years ago is so vague that I must ask you to complete the picture for yourselves by throwing in any other accessories which may occur to you as giving it a strong eighteenth-century flavour,

such as a few link-boys, a solitary watchman, an oil lamp or two, and a plentiful sprinkling of puddles and mud. You are informed that the house belongs to Sir Joseph Banks, who is the President of the Royal Society of London, and that the occasion is one of his weekly conversaziones. The portly visitor, with the large frill, makes his way upstairs, to the evident embarrassment of a thin, middle-aged gentleman in an old-fashioned Court dress of faded violet and a knocker-tailed periwig, who is moving uneasily about on the landing, manifestly afraid to face the assembly. The approach of the gentleman on the stairs, however, drives him into the room. He shuffles quickly from place to place, his manner is awkward ; his face betrays a nervous irritation of mind, and he appears annoyed if looked at. It is the Honourable Mr. Cavendish. Finding himself close to a group, evidently, from the appearance which their faces wear, speaking of a deeply important matter, he draws near to listen. They are talking of a rumour of some grave disaster which has befallen my Lord Cornwallis and his troops, who it would seem have been circumvented in some unexpected manner by the machinations of that arch-rebel Washington. Mr. Cavendish is scarcely interested, and he moves aside to catch something concerning, it may be, some fresh eccentricity of poor Lord George Gordon, or perhaps some account of the troubles of the unhappy Mr. Watt, the engineer, who, it is being said, is fighting tooth and nail to defend his just rights from a set of unprincipled rogues who pirate his inventions. None of these matters is sufficiently moving to detain him. But his manner quickly alters when he overhears the mention of the name of Mr. Herschel. Mr. Herschel is a musician at Bath, who employs his leisure in constructing big telescopes, with one of which

he has just discovered a new planet. Mr. Cavendish is greatly interested; he listens with marked attention; he is even about to put a question, and begins in a nervous, hesitating manner, and in a thin, shrill voice, when his eye catches that of a stranger; he is instantly silent, and retires in great haste, for he has a horror of a strange face. The portly gentleman with the large frill espies him, and comes up with a foreign gentleman, who is formally introduced to Mr. Cavendish. Mr. Cavendish is assured by the portly gentleman that his foreign friend is particularly desirous to make the acquaintance of a philosopher so profound and so universally celebrated—all of which is confirmed by the foreign gentleman, who adds that it was, indeed, his chief reason for coming to London, that he might see and converse with one of the greatest ornaments of Britain, and one of the most illustrious philosophers of that or any other age. Mr. Cavendish is speechless; he is overwhelmed with confusion, until, seeing an opening in the crowd, he darts through it with all possible speed, and, reaching his carriage, is driven home. His house is precisely such as you would expect from one of his habits and disposition; it is made up of laboratories and workshops, and very little is set apart for personal comfort. The principal laboratory is in what the builder intended to be the drawing-room; in an adjoining chamber is a forge; and the upper apartments are turned into an astronomical observatory. Mr. Cavendish rarely did violence to his love of solitude by asking any one to his house. If a friend chanced to dine with him, he was invariably treated to a leg of mutton, and nothing else. We are told that on one occasion, three or four guests being expected, he was asked what was to be got for dinner. He replied with the customary formula, "A

leg of mutton." "But," said the servant, "that will not be enough for five." "Then get two legs," was his answer. During the latter part of his life Mr. Cavendish was immensely rich. At the time of his death he was said to be worth a million and a quarter, and was the largest holder of Bank Stock in England. But he who was said to be the most wealthy of learned men, and the most learned of wealthy men, seemed quite indifferent to his riches. There is a well-known story of his threatening to remove his money out of the hands of his bankers if, as he said, they continued to plague him about it. Cavendish, as you may suppose, could never be induced to sit for his portrait; but an artist, who was bent upon having it, managed to get near his subject unobserved, and first sketching the three-cornered hat, and then the greatcoat, he patiently watched his opportunity and inserted the profile between them. This, I believe, is the only known or authentic portrait of Cavendish.

The life of such a man is, as you may well imagine, nearly devoid of incident. There is but little more of his personal history to tell, except that he was the son of Lord Charles Cavendish, that he was born at Nice in 1731, and that he died in London in 1810. He died as he had lived, voluntarily severing every tie of human sympathy. When he found himself near his end, he called his servant to his bedside, and said, "Mind what I say—I am going to die. When I am dead, but not till then, go to Lord George Cavendish and tell him—Go!" The dying man wished to be alone, and the servant, who hesitated to leave him, was ordered from the room. In half an hour he returned to find that his master had turned his face to the wall, and quietly passed away.

There is nothing lovable in such a character ; on the other hand, there is nothing in it that is despicable. This passionless man, whose moral character seemed almost a blank, had a marvellously clear intelligence, and a range of mental vision second to none of his age. In extent of acquirements, and in profundity of learning, he was unsurpassed by any of his contemporaries. His published work, although of the highest order, gives a very incomplete idea of his powers. He left behind him a mass of papers which indicate that he was far in advance of the science of his time. His memoirs on heat and electricity contain the germs of discoveries, if not actual discoveries, which are commonly associated with the names of subsequent investigators. He was an accomplished practical astronomer and a profound mathematician. His knowledge of the calculus and the manner in which he handled it have been described as masterly.

Science is indebted to a learned Scotch professor of the eighteenth century—Dr. Black—for the discovery of certain fundamental laws of heat ; and the elucidation of these laws seems to have been the subject of Cavendish's earliest inquiries. One of the problems he set himself to solve, in the course of these investigations, was whether our mercurial thermometer was an accurate and uniform measurer of temperature, to the extent of showing whether the temperature of a mixture of hot and cold water is the mean of the temperatures of the hot and cold water before mixing. Having found that such was the case, Cavendish proceeded to determine the effect of mixing dissimilar liquids at different temperatures. "One would naturally imagine," he says, "that if cold mercury, or any other substance, is added to hot water, the heat of the mixture would be the same as if an equal

quantity of water of the same degree of heat had been added, or, in other words, that all bodies heat and cool each other when mixed together equally in proportion to their weights."

He then shows by experiment that such is not the case. He mixed quicksilver and water together at different temperatures, and found that if it required 1 lb. of water at a known temperature to cool a certain weight of hot water through a certain number of degrees, it would require 30 lbs. of quicksilver to cool the same weight of hot water through the same interval of temperature. He made trials with various metals, with sulphur, glass, charcoal, and many other bodies, and he concludes "that the true explanation of these phenomena seems to be that it requires a greater quantity of heat to raise the heat of some bodies a given number of degrees by the thermometer than it does to raise other bodies the same number of degrees."

We have here the first clear enunciation of a very important matter: if Cavendish had communicated his discovery to the world when he made it, namely in 1764, he would have had priority over those who are generally styled the discoverers of the fact of specific heat.

Cavendish did much to improve the mercurial thermometer. He pointed out several sources of error in the methods of making and using it. He was the first to insist on the necessity of correcting its indications when the whole of the mercury is not within the space of which the temperature is to be ascertained, and the first to draw up special directions to ensure uniformity in the mode of graduating it. He also accurately determined the temperature at which quicksilver freezes, and found it to be 39 degrees below the point at which water

is ordinarily turned into ice. But it would require an entire evening to tell you all that Cavendish did on the subject of heat. That it occupied much of his attention is obvious from the number and character of his experiments, and the excellence of his numerical results. It is evident, too, that he thought deeply on the nature of heat. He rejected the doctrine that it was material, rather holding, as he tells us, "Sir Isaac Newton's opinion, that heat consists in the internal motions of the particles of bodies"; the theory in fact which is now, I should suppose, universally current. And it is worthy of remark that one of the greatest exponents of this theory was the director of one of the finest physical laboratories in the world—a laboratory erected at Cambridge to the memory of Cavendish by his descendant, the late Duke of Devonshire.¹

Cavendish was a natural philosopher in the widest sense of the term, for he occupied himself in turn with every branch of physical science known in his time. But it is to his discoveries in chemistry that his fame is chiefly due; and here again we may trace the influence of Black in directing the current of his early inquiries. Chemists, up to the middle of the eighteenth century, had no clear conception of the existence of a variety of gaseous substances perfectly distinct from one another. They were inclined to believe that all the different forms of gas they met with were merely modifications of one and the same substance. Their distinctive characters were supposed to arise from their being "tainted," or "infected with fumes, vapours, or sulphurous spirits." The publication of a celebrated essay by Black on "*Magnesia Alba*" marked an epoch in the history of chemistry by demonstrating the existence of at least one gaseous

¹ The late Professor Clerk Maxwell.

body totally distinct from the air we breathe. Black showed that the difference between chalk and quicklime was due to the presence of a gas in the chalk which was not in the quicklime. Quicklime, indeed, had the property of fixing this air, and of thus being converted into chalk. Black named this air, which was so capable of entering into the composition of bodies, "fixed air"; nowadays we call it carbon dioxide, a name which denotes its composition, of which Black was ignorant. Black did very little towards investigating this gas in the free state. The first full account of its properties was given by Cavendish in 1766. Cavendish prepared the fixed air with which he experimented by dissolving marble, which is, chemically speaking, the same thing as chalk, in spirits of salt, or hydrochloric acid. He found that the gas dissolved in its own bulk of water at common temperatures, and that cold water dissolves more of it than hot water; indeed, he says, "water heated to the boiling point is so far from absorbing the air that it parts with what it had already absorbed." Lime and alkalis, especially if dissolved in water, rapidly absorb the gas, but it may be collected and preserved over quicksilver for any length of time; indeed chemists owe the idea of using quicksilver to collect and preserve certain gases which are absorbed by water to Mr. Cavendish.

Although you are blessed here in Manchester with one of the best water supplies in the kingdom, you doubtless have heard of what are called "hard" waters; you may even know that some of these hard waters are made "soft" by boiling, and that the kind of hard water which is softened by boiling deposits a crust or "fur" in the tea-kettle, and a "cake" in the steam-boiler. Now this "fur" is mainly composed of chalk,

kept in solution in the water by the fixed air dissolved therein. When the water is boiled the fixed air is expelled, as Cavendish tells us, and accordingly the chalk is deposited. This explanation of the origin of the "fur" was first given by Cavendish. Possibly some of you may know that such hard waters are frequently softened on the large scale by adding lime to them. The lime combines with the fixed air (the agent, you bear in mind, which keeps the chalk in solution), and accordingly the chalk is deposited, together with that formed by the union of the fixed air with the added lime. The fact that water could be thus deprived of its dissolved chalk was pointed out by Cavendish. When the carbon dioxide is allowed gradually to escape from the solution, the carbonate of lime is deposited in small crystals, the shapes of which are often exceedingly curious and beautiful; indeed, there is no substance which has such a diversity of crystalline form as this carbonate of lime.

In various parts of the world, particularly in districts where limestone abounds, there are large caves, or grottoes, from the roofs of which depend long icicle-shaped masses of carbonate of lime termed *stalactites*. If you notice one of these masses you will observe that occasionally a drop of water falls from the end of it to the floor, or rather upon a similar mass of carbonate of lime on the floor, exactly underneath that which hangs from the roof. The lower mass, which appears to stretch up towards the upper one, is termed a *stalagmite*. Occasionally the two masses meet one another and unite to form a continuous column. The origin of these masses—these stalactites and stalagmites—will readily occur to you: the rain-water percolating through the rock above the cave contains carbonic acid in solution,

by which it dissolves the carbonate of lime in the rock. As it drips from the roof it gives up a portion of its carbonic acid to the air in the cavern, and accordingly a portion of the carbonate of lime is deposited; the next drop runs over the mass so deposited, and by giving out another portion of dissolved carbonic acid deposits another portion of carbonate of lime on the first deposition; and so the process goes on, each portion of water from the roof running down the icicle of carbonate of lime which is formed, and continually adding to its length. But the drops fall off to the floor long before they have given up the whole of their carbonic acid, and therefore long before they have yielded up all the chalk which they held in solution. Accordingly the escape of the carbonic acid goes on from the water after it has fallen on the floor, and so you get this second deposit of carbonate of lime—this stalagmite—formed underneath the stalactite.

Cavendish also showed that fixed air was considerably heavier than common air by weighing a bladder filled first with the one gas and then with the other. The fixed air he found to be one and a half times heavier than the common air.

The old chemists, who in days gone by greatly busied themselves to discover a more direct method of turning things into gold than is practised by their successors in the chemical arts, have left us some marvellous stories concerning the behaviour of a gas which seems to be evolved from certain metals when they are brought into contact with acids, such as oil of vitriol, or muriatic acid. The exact nature of this gas remained unknown until Cavendish investigated its properties. This gas, which we now call hydrogen, is highly inflammable, and Cavendish showed that, like many other inflammable

bodies, it cannot burn without the assistance of common air. When mixed with rather more than double its volume of air, it explodes violently on the approach of a light. He also weighed this gas by the same method which he had employed to weigh the fixed air, and he found it to be eleven times lighter than common air. Cavendish, however, underestimated the lightness of this gas; in reality it is about fourteen and a half times lighter than air.

When giving you an account of Priestley's work, I described to you his method of analysing the air. It was based on the fact that when the gas known as *nitric oxide* comes in contact with air, the oxygen in the air combines with the nitric oxide to form a product soluble in water. If the mixture of gases is made in a tube standing over water, the diminution in volume, consequent on the removal of the oxygen, is a measure of the amount of that gas in the air. As the quality of the air was supposed to depend upon the diminution of volume which it suffered by being mixed with nitric oxide, the instruments designed to make the tests were termed *eudiometers*, from two Greek words denoting a "measure of goodness." Without going into details, I may say that this method of analysis is liable to an objection from the cause first worked out by our illustrious townsman, John Dalton, that the same volume of oxygen can combine with different volumes of the nitric oxide. This fact was indeed known to Cavendish, and he made a great number of experiments in order to ascertain the best method of mixing the gases so as to obtain constant results.

By means of the apparatus he devised he was enabled to show that the composition of the atmosphere is sensibly constant. He tells us that "during the last

half of the year 1781 I tried the air of near sixty different days . . . but found no difference that I could be sure of, though the wind and weather on those days were very various, some of them being very fair and clear, others very wet, and others very foggy." This conclusion is in harmony with the results of later experimenters. The atmosphere has practically the same composition all the world over, and all the year round. Although there are slight variations in the relative proportion of the constituents, methods of the highest precision are required in order to detect them. Cavendish gives us the numerical results of his experiments, and from these it appears that, when expressed in the manner we now adopt, the mean composition of the air is in 100 parts by measure :—

Oxygen	20·8
Nitrogen	79·2

The most refined analytical methods of modern times have shown that the average numbers are

Oxygen	20·9
Nitrogen	79·1

A result, you see, almost identical with that deduced from Cavendish's observations, and one which illustrates in a very striking manner the extreme care and accuracy with which he worked.

Cavendish next proceeded to determine the cause of the diminution in volume which common air occasionally suffers when substances are caused to burn in it.

Among the many experiments which he made in order to elucidate this matter there is one which is especially remarkable, as it led him to his greatest discovery, that of the composition of water—a discovery which will make the name of Cavendish for ever memorable.

Dr. Priestley relates in one of his volumes of *Experiments and Observations on Air*, that when a mixture of common air and inflammable air is exploded by the electric spark in a glass vessel, "the inside of the glass, though clear and dry before, immediately became dewy." "As this experiment," says Cavendish, "seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely." Cavendish repeated this experiment in his characteristically careful manner. The inflammable air and common air were mixed in varying but known proportions, and the diminution in volume which attended the explosion was accurately noted in each case, and the amount of oxygen remaining in the air was determined by the eudiometer. Cavendish found that the greatest diminution of volume occurred when two volumes of hydrogen were mixed with five volumes of air.

He tells us that when this mixture is exploded, "almost all the inflammable air and about one-fifth part of the common air lose their elasticity, and are condensed into the dew which lines the glass." Cavendish continues: "The better to examine the nature of this dew 500,000 grain measures of inflammable air were burnt with about two and a half times that quantity of common air, and the burnt air made to pass through a glass cylinder 8 feet long and $\frac{3}{4}$ inch in diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable air nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. . . . By this means upwards of 135 grains of water were condensed in the cylinder,

which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; in short, it seemed pure water. . . . By the experiments with the globe it appeared that when inflammable air and common air are exploded in a proper proportion, almost all the inflammable air and near one-fifth of the common air lose their elasticity, and are condensed into dew. And by this experiment it appears that this dew is plain water, and consequently that almost all the inflammable air and about one-fifth of the common air are turned into pure water."

Cavendish then repeated the experiment with pure oxygen, or "dephlogisticated air," as this gas was then termed. I will give you the result in his own words, for the account has a great historical interest: "I took a glass globe holding 8800 grain measures, furnished with a brass cock, and an apparatus for firing air by electricity. This globe was exhausted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar, inverted in water, and containing a mixture of 19,500 grain measures of dephlogisticated air, and 37,000 of inflammable; so that on opening the cock some of this mixed air rushed through the bent tube and filled the globe. The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded

in it, without any fresh exhaustion of the globe." Cavendish, however, found that in many of his trials the condensed water was sensibly acid to the taste, and by saturation with alkali, and evaporation, it yielded nitre. The search for the cause of the formation of this acid led Cavendish to another discovery—namely, that of the composition of nitric acid, an acid which is probably familiar to you under its old name of spirits of nitre or *aqua fortis*. He showed that the formation of this acid was not an essential part of the process of the union of the oxygen and hydrogen, but that it was due to the presence of impurities in the gases used. Whenever the amount of oxygen was larger than could combine with all the hydrogen in the mixture, a portion of that oxygen united with the nitrogen of the common air present, and so formed the nitric acid.

Such, then, were the experiments which led to the discovery, firstly, of the compound nature of water; secondly, of the character of its constituents; and thirdly, of the proportions in which these constituents are combined together. It would be impossible to over-estimate the value of this discovery: it marks one of the great epochs in the history of chemistry. Who could have predicted that this most familiar of all liquids—a liquid, too, long regarded as the very type of a chemical element—was composed of two colourless invisible gases—the one the inflammable hydrogen, the lightest substance known—the other, oxygen, the life-sustaining principle in the air we breathe—nay, the element which has been styled "the chemical centre in the scheme of nature"?

Nearly every important discovery has to pass through two ordeals—it is first impugned as not true, and then as not new; and this great discovery which I have

ascribed to Cavendish formed no exception to this rule. Not many years ago there was a great controversy concerning the question—Who was the discoverer of the composition of water? I am not now going to rake up the matter, for it is gradually being forgotten; but I think that every chemist now allows that the claims of Cavendish have been incontestably proved. The fact is the time was ripe for this discovery. Everybody familiar with the chemical work of the latter half of the eighteenth century will admit that the labours of a dozen of Cavendish's contemporaries were tending more or less directly to the same goal, and had Cavendish proved unequal to his opportunities, his grandest discovery would not have been long delayed. It has been said that the discovery of law is regulated by law, and the history of the discovery of the composition of water affords a striking exemplification of the truth of this remark.

The time will scarcely allow me to tell you more of what Cavendish did; but, if I am not trespassing too much on your patience, I should like just to mention another great work of his, since any account of Cavendish's labours would be very incomplete without some reference to it. An ancestor of Cavendish's was one of the first to sail round the earth. Cavendish himself was one of the first to attempt to weigh it. Cavendish, in fact, undertook to determine how much heavier the earth is than a spheroid of water of equal size. The apparatus which he employed consisted of a long light wooden rod suspended horizontally by a thin wire. At the ends of the rod were leaden balls about two inches in diameter, and near these could be brought two large spherical masses of metal. By the mutual attraction of the balls, big and little, the long rod was caused to move slightly. The amount of the deviation, and the force

necessary to produce it, being known, together with the weight of the balls, and the distances from their centres, the attraction of a spheroid of water of the same diameter as the earth upon the ball on its surface can be calculated, from which can also be calculated the relation of the earth's density to that of water. From his experiments, Cavendish concluded that the earth is about five and a half times heavier than water—a result which the subsequent labours of Mr. Baily, made with extraordinary care and patience, have shown to be very near the truth. It deserves to be mentioned, however, that Newton, with that marvellous insight which nowadays seems to us nothing less than divination, had predicted that the earth would be found to be between five and six times heavier than water.

One more remark and I have done. A celebrated French chemist, whose patriotism we admire scarcely less than his genius, has declared that "Chemistry is a French Science, its founder was Lavoisier, of immortal memory." The merit of Lavoisier is undoubtedly great, and the influence which he exerted on the development of chemistry was profound. It is accounted the chief glory of Lavoisier that he first clearly pointed out that the principles of gravitation lie at the basis of chemistry; that chemistry is in fact a science of quantitative relations. But let us take a retrospect of Cavendish's labours. He fixed the weight of the earth; he established the proportions of the constituents of the air; he occupied himself with the quantitative study of the laws of heat; and lastly, he demonstrated the nature of water and determined its volumetric composition. Earth, air, fire, and water—each and all came within the range of his observations. Now, I ask you, what is the most obvious characteristic of all this labour? Is it not its

thoroughly quantitative character? Weighing, measuring, calculating; such, indeed, was pre-eminently the essential nature of Cavendish's work. If, then, the claim of any one to be styled the founder of chemistry as a science rests upon his recognition of its quantitative relations, may we not also, and with equal truth, say that "Chemistry is an English Science—its founder was Cavendish, of immortal memory"?

V

JAMES WATT

AND

THE DISCOVERY OF THE COMPOSITION OF WATER

BEING THE WATT ANNIVERSARY LECTURE DELIVERED BEFORE THE
GREENOCK PHILOSOPHICAL SOCIETY ON 11TH MARCH 1898

WHEN your Secretary did me the honour to communicate the wish of the Committee that I should deliver this lecture, he was good enough to send me a list of the names of my predecessors in the position I was invited to occupy, together with a statement of the subjects on which they had addressed you. I confess I read his letter with very mingled feelings. To be asked to form one of such a distinguished company was in itself an honour which I deeply appreciated. On the other hand, it seemed well-nigh hopeless to find any theme associated with the life and work of the great man whose services to humanity we are this day called upon to commemorate that had not been dealt with by one or other of those who had preceded me. Naturally, and as befits the subject, the greater number of those who have spoken on these occasions have been distinguished engineers and mechanicians, and they have been able to speak with a fulness of knowledge and a weight of authority on the outcome of the great engineer's labours to which I,

who know nothing of engineering or machinery, can have no pretensions.

It has occurred to me, however, that there might be one incident in Watt's career which, in all probability, had not been handled by any one of those whom you have invited to appear here, and on which, as it comes within my own province, I thought I might venture, without presumption, to engage your attention. I was the more impelled to select it, in that it illustrates one side of Watt's intellectual activity which those who regard him only as an inventor and a mechanic are apt to undervalue, or even to lose sight of altogether. It serves, too, to throw additional light upon his mental character and his moral worth, and thus enables us to form a fuller and more just appreciation of the attributes of the man we wish to honour. The incident, in a word, relates to Watt's share in the establishment of the true view of the chemical nature of water.

To the historian of science this is doubtless an old story on which it would be difficult to say anything new. The literature concerned with it occupies many volumes, largely owing to the circumstance that it has given rise to a controversy which has engaged the active interest of some of the strongest and subtlest intellects of the nineteenth century. Some of the disputants have been men like Brougham, Jeffrey, and Muirhead, skilled in the arts of advocacy and in the faculty of eliciting and weighing evidence, who have stated their conclusions with all the "pomp and circumstance" of a judicial finding; others are men like Arago, Dumas, Harcourt, Whewell, Peacock, Kopp, George Wilson, eminent in science and literature, who have defended their convictions with great power, ample knowledge,

much argumentative force, and occasional eloquence. At one time the contest was waged with no little fury and bitterness; it threatened, indeed, like the famous controversy as to the proper form of a lightning-conductor, during Sir John Pringle's presidency of the Royal Society, or like the equally famous controversy as to the true discoverer of the planet Neptune, to attain the dignity of a national question, far more acute, I should imagine, than that which has recently occasioned all right-feeling Scotchmen to approach the Queen in Council on the subject of Scotland's proper place and designation in Imperial concerns.

But, happily, the acrimony and ill-feeling have long since passed away. There is no longer any need to discuss the question either as an advocate or as a partisan. What I shall attempt to-night is to treat it dispassionately, and, within the compass of an hour, to assess, as impartially as I am able, Watt's true place in regard to this discovery.

It was indeed an epoch-making event. The discovery of the composition of water was as momentous for science as the greatest of Watt's inventions was for social and economic progress. The very fact itself, apart from all that flowed from it, was of transcendent interest. But to those who had eyes to see, its supreme importance was in its fruitful and far-reaching consequences. It signified nothing less than the passing away of an old order of things: the downfall of a system of philosophy which had outlived its usefulness, in that it no longer served adequately to interpret natural phenomena, and had become rather a hindrance and a stumbling-block to the perception of truth. The discovery at once led to the inception of a more rational and more truly comprehensive

theory, which not only explained what was already known in a fuller, clearer, and more intelligible manner, but pointed the way to new facts hitherto undreamt of,—facts which in their turn served to strengthen and extend the generalisation which led to their discovery. No wonder, therefore, that those who loved and revered Watt, and who were rightly jealous of his honour, should have sought to do all in their power to vindicate what they honestly conceived to be his just title to so signal and so fundamental a discovery.

No man has a juster claim to be regarded as a scientific man, in the truest and noblest sense of that term, than James Watt. The scientific spirit was manifest in him even in boyhood. The very circumstances of his condition, his weakly frame, the solitariness of his school-life, and the early habit of introspection thus induced in a mind forced to feed only on itself, served to strengthen and develop the instinct. Even his early struggles, and the jealousy of the Glasgow Guilds, which forbade him to practise his trade in the burgh in which he had not served an apprenticeship, conduced to mould his character and to determine the bent of his mind. Hard and illiberal as it seemed at the time, the *Zunftgeist* which drove him to the shelter of the old College in the High Street, and secured for him the abiding friendship of Black and Robison, was in reality the most fortunate circumstance of his career. It brought him directly under the influence of one of the greatest natural philosophers of his age, and stamped him permanently as a man of science. It would not be difficult to trace how this influence reacted upon all that Watt subsequently did—from the time of his earliest speculations on the loss of

energy in Newcomen's engine down to the very last of his mechanical pursuits in the dignified retirement of Heathfield Hall. He approached the question of the improvement of the steam-engine as a scientific problem, and under the direct inspiration of the doctrine of the great discoverer of the principle of latent heat. It was this same mental attitude towards scientific truth, the same receptivity for scientific doctrine, the same love of pondering over and speculating upon the true inwardness of things, that brought him the friendship of Priestley, Withering, Wedgwood, and Deluc, and that ultimately made him a cherished member of the foremost scientific academies of the world. It will occasion little surprise to one who has formed a true perception of his character to learn that Watt was wont, even at periods of great mental depression, and of physical suffering, amidst all the toil and anxious worry of a business surrounded with difficulties, to find peace in the contemplation of natural phenomena, and to spend time in philosophical speculation. The shrinking, diffident man, in thus communing with himself and with Nature, followed a true and constant impulse to withdraw from the strife and turmoil of the world, and to seek his pleasure and his rest in the contemplation of natural truth. No one can look upon that contemplative face without being struck with its expression of philosophic calm. What deep, genuine pleasure these communings brought to the harassed man may be gleaned from his correspondence. In truth, Nature intended Watt to be a philosopher of the pattern of Boyle, or Newton, or Dalton: it was destiny that drove him into the world of affairs, where, as he said, he was out of his true sphere. It is necessary to dwell for a moment on this

aspect of Watt in order to form a just appreciation both of his position and of his merits in regard to the great chemical truth with which his name is associated. The man of action is apt to regard the contemplative mind with something akin to contempt. I once heard a bustling, busy man, the head of a large engineering establishment, who had enjoyed the good fortune to be a pupil of Thomas Graham, say of that distinguished philosopher that he was the laziest man he ever met. He did not say he ever knew—for how little he really knew Graham was evident from the fact that at the period to which he referred Graham's thoughts were deeply occupied with some of the most memorable of his investigations.

It was in one of these contemplative moods—in what he himself styled his periods of excessive indolence—and, as it happened, at the very time that the Soho firm was struggling to protect itself against the unprincipled horde that was seeking to infringe Watt's fundamental patent, that he occupied himself with turning over in his mind the outcome of one of his friend Priestley's multitudinous experiments. Watt had long held the view that air was a modification of water, or, as he expressed it in a letter to his friend Black, under date Dec. 13, 1782, that, as steam parts with its latent heat as it acquires sensible heat, or is more compressed, when it arrives at a certain point it will have no latent heat, and may, under proper compression, be an elastic fluid nearly as specifically heavy as water; at which point, he conceived, it would again change its state and become air. As he then relates, he sees a confirmation of this opinion in an experiment of Priestley's, made, as he says, "in his usual way of groping about." "As he [Priestley] had

succeeded in turning the acids into air by heat only, he wanted to try what water would become in like circumstances. He undersaturated some very caustic lime with an ounce of water, and subjected it to a white heat in an earthen retort. . . . No water or moisture came over, but a quantity of air, equal in weight to the water, . . . a very small part of which was fixed air, and the rest of the nature of atmospheric air. . . . He has repeated the experiment with the same result."

About a fortnight later Priestley wrote that he was able to convert water into air "without combining it with lime or anything else, with less than a boiling heat, in the greatest quantity and with the least possible trouble or expense." He added that "the method will surprise more than the effect," but that he would defer "the communication of the hocus pocus of it" until such time as Watt should give him the pleasure of his company in return for the pleasure he was to give Watt in speculating on the subject.

These experiments, as we shall see in due course, were wholly fallacious: in following them up with his wonted ardour Priestley quickly found himself in a maze of contradictions, and ultimately discovered that this seeming conversion was absolutely mythical.

It may be useful, however, to make one or two comments on these passages at the present juncture. In the first place, Watt's opinion as to the relations of water and air, although founded as he thought upon a more philosophical basis, simply embodied the teaching of the schoolmen. The notion that the so-called four elements were mutually convertible, or were in essence identical, ran through the doctrine of twenty centuries of teachers. Despite the onslaughts of the Spagyrist, and of the author of the *Sceptical Chymist*, it per-

meated the literature of natural philosophy down to the very beginning of this epoch. Watt was insensibly swayed by a belief which had descended to him, like the undying germ, through the ages, and he could no more shake himself free of it than he could get rid of the influence of heredity. The very mode in which he, in common with the men of his time, uses the term "air," is an indication of the manner in which this ancient creed limited and cramped his thought. He knew that there were various "airs," but it is very doubtful if he realised that they were essentially different substances. There is abundant evidence in the few chemical papers that he published, and especially in his letters to Black, Priestley, Deluc, Kirwan, and others, that he regarded them all as constituted of the same matter, affected by attributes more or less fortuitous and accidental. Thus, all the varieties of inflammable air were at bottom identical, with properties modified by their origin, or by their varying content of the hypothetical principle phlogiston—that is, the principle that was assumed to make them burn.

From Watt's published correspondence we are able to judge how he regarded Priestley's further work on this so-called conversion of water into air. He admits that the facts are "in some degree contradictory to each other." The apparent conversion would seem to depend upon the material of the vessel in which it was made. In a glass vessel no air was produced, nor was any found in a gun-barrel when the distillation was done slowly, but when confined by a cock, "and let out by puffs it produces much air; which," says Watt, "agrees with my theory and also coincides with what I have observed in steam-engines. In some cases I have seen the tenth of the bulk of the water, of air extricated or made from

it." Davy once said "the human mind is always governed, not by what it knows, but by what it believes, not by what it is capable of attaining, but by what it desires." However willing to catch at anything in support of his belief, it is possible that Watt might have been led to doubt the soundness of Priestley's experiment if an apparent and wholly unlooked-for confirmation of it had not now arisen.

To make the account exact, and in view of what is to follow, it is necessary to go back a little, in point of time. In the spring of 1781 Priestley performed what he styled "a mere random experiment made to entertain a few philosophical friends." It was practically a repetition of Volta's experiment of firing a mixture of the inflammable air from metals, that is, hydrogen, with common air in a closed glass vessel by means of the electric spark. After the deflagration the vessel was found to be hot, and on cooling its sides were observed to be bedewed. Neither Priestley nor any one of his philosophical friends seems to have paid any particular attention to the deposit of moisture, or at all events if they did they failed to perceive its significance. One of them, however, Mr. John Warltire, a Lecturer in Natural Philosophy in Birmingham, imagined that the experiment might afford the means of showing whether heat was ponderable or not, and accordingly he repeated it, using for greater safety a copper globe, weighed before and after the passage of the spark. A minute loss of weight was always noticed, "but not constantly the same: upon the average it was about two grains."¹

Priestley, who, with Withering, was present when

¹ The account of these experiments is given in a letter to Priestley, and constitutes No. V. of the Appendix to Priestley's *Experiments and Observations* relating to various branches of Natural Philosophy, etc. Vol. II., Birmingham, 1781.

the experiments were made, confirmed the apparent loss of weight; but he adds, with a caution that was not habitual, that he did not think "that so very bold an opinion as that of the latent heat of bodies contributing to their weight should be received without more experiments, and made upon a still larger scale."

Priestley's volume—the sixth in the series—was published in 1781, and was certainly known to Watt; indeed, in the Appendix are printed a number of observations made by him, apparently as the work was passing through the press. Although, therefore, he must have had his attention drawn at about this time to the formation of the dew in Priestley and Warltire's experiment, there is nothing to show that he attached any importance to the circumstance, or that if he did he dissented from Warltire's conclusion that common air deposits its moisture when it is phlogisticated.

For some time previous to the publication of Priestley's book, Mr. Cavendish was engaged upon an inquiry "to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed." In other words, it was an investigation to determine the changes experienced by air when bodies were made to burn in confined portions of it. On the appearance of Priestley's book he repeated Warltire's experiment, thinking "it worth while to examine more closely" as it "seemed likely to throw great light on the subject I had in view." He confirmed the observation on the formation of dew, but although he made the experiment on a large scale, and with varying proportions of the two airs, he was unable to satisfy himself as to the loss of weight after the explosion. As the result of a number of trials made both with the

inflammable air from zinc and from iron, that is, hydrogen, and mixed with common air in the proportion of 423 measures of the inflammable air to 1000 of common air, he says, "we may safely conclude that when they are mixed in this proportion and exploded, almost all the inflammable air and about one-fifth part of the common air lose their elasticity and are condensed into the dew which lines the glass." In order to examine the nature of this dew, large quantities of the hydrogen were burnt with two and a half times its volume of common air, and the product of the combustion was caused to pass through a long glass tube whereby it was condensed. "By this means 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water. . . . By the experiments with the globe it appeared that when the inflammable and common air are exploded in a proper proportion, almost all the inflammable air and near one-fifth of the common air lose their elasticity, and are condensed into dew. And by this experiment it appears that this dew is plain water, and, consequently, that almost all the inflammable air and about one-fifth of the common air are turned into pure water."

The idea that common air was for the most part a mixture of two gases—oxygen, or the dephlogisticated air of Scheele and Priestley, and nitrogen, or the mephitic air of Rutherford, the azote of Lavoisier—was familiar to chemists at this period as the result of the teaching of Scheele and Lavoisier; and there is reason to suppose that this opinion was shared by Cavendish. He had been engaged for some time past in an elaborate inquiry into the constitution of atmospheric air, the

results of which admitted of no other interpretation than that common air was composed of two different gases, mixed or combined in constant relative proportions. It is true that in the memoir containing the results of his inquiry he nowhere directly gives his estimate of these relative quantities, but from the data he affords it is easy to deduce both the amount and the constancy of the proportion. Cavendish's papers are characterised by a remarkable conciseness and brevity ; an experiment which must have involved the putting together of elaborate and complicated apparatus, and which must have occupied considerable time in its performance, is described in a few lines, and hence it is not always possible to gather with certainty the precise disposition of the arrangements. He never sets out his reasons or his conclusions with any great amount of detail, and his published works occasionally give little indication of his line of thought. But that he clearly recognised that only one portion of common air was concerned in the formation of water, and that this portion was the dephlogisticated air, or oxygen, is obvious from his next series of experiments, in which he fired a mixture of about two measures of hydrogen and one measure of oxygen in a previously exhausted glass globe, furnished with an apparatus for firing air by electricity. When the included air was fired, almost all of it lost its elasticity, so that fresh quantities of the explosive mixture could be introduced and the process repeated until a sufficient quantity of the moisture was obtained for examination. In these experiments Cavendish clearly and definitely demonstrated that the weight of the water was practically equal to the weight of the mixed gases which had combined to form it. In some cases the water was perfectly neutral in its reaction ; in

others' it was slightly acid, and the cause of this acidity cost Cavendish much experimenting to discover, but he is never in any doubt as to the main result; he says distinctly, "if those airs could be obtained perfectly pure, the whole would be condensed." Now, if Cavendish had published this main result at the time he obtained it, namely, in the summer of 1781, or even if he had formally communicated it to one of the meetings of the Royal Society during the ensuing session, there would have been no Water Controversy. But even if he were ready, it was characteristic of him to delay, not from inertia or indolence, but from a morbid shyness, an unconquerable reticence, which constantly led him to postpone any public announcement of his work. He had the additional, and to him all-sufficient reason, that he had not yet worked out the cause of the occasional acidity of the water. What he did, however, was to communicate the facts of his experiments to Priestley, as Priestley himself states in a subsequent paper published in the *Philosophical Transactions* for 1783. When or how he communicated them to Priestley does not appear, nor have we any means of knowing precisely what was said. Something, however, on this point may be inferred from what Priestley proceeded to do. It appears from a letter to Wedgwood that he repeated Cavendish's experiments during the March of 1783. It will be remembered that he was at this period engaged on his experiments on the seeming conversion of water into air. He had obtained a number of contradictory results, which had led Wedgwood, as far back as the previous January, to put certain sagacious queries which doubtless in the end had their effect in opening Priestley's eyes to the origin of his mistake. But at the time both he and Watt were seeking for fresh evidence

to substantiate the possibility of this conversion. Now just as Cavendish thought that Warltire's experiment might throw light upon the particular matter on which he was then engaged, so Priestley considered that Cavendish's work might afford evidence—indirect, it is true, but still evidence—of the intimate connection between water and air. Cavendish had, he thought, established the converse of the proposition which he and Watt were seeking to prove, in showing that "air," or rather certain kinds of "air," could be converted into water, weight for weight. It was no longer the original Warltire experiment of exploding common air and hydrogen. Cavendish had indicated the particular kinds which were really concerned in the phenomenon, and it was the Cavendish experiment, pure and simple, that Priestley proceeded to repeat. This is obvious from what he says: "Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the *reconversion* of air into water by *decomposing* it in conjunction with inflammable air." Priestley here uses the word "decomposing," in a sense contrary to that which the context implies, but that he is consistent in so using it is evident from what follows, and also from similar expressions to be found in his correspondence. But although he professed to repeat Cavendish's experiment, he neglected to do so in Cavendish's manner. He says: "In order to be sure that the water I might *find in the air was really a constituent part of it*, and not what it might have imbibed after its formation [*i.e.* by contact with the water of the pneumatic trough], I made a quantity of both dephlogisticated and inflammable air, in such a manner as that neither of them should ever come into contact with water, receiving

them as they were produced in mercury; the former from nitre, and in the middle of the process (long after the water of crystallisation was come over), and the latter from perfectly made charcoal. The two kinds of air thus produced I decomposed by firing them together by the electric explosion, and found a manifest deposition of water, and to appearance in the same quantity as if both the kinds of air had been previously confined by water. In order to judge more accurately of the quantity of water so deposited, and to compare it with the weight of the air decomposed, I carefully weighed a piece of filtering-paper, and then having wiped with it all the inside of the glass vessel in which the air had been decomposed, weighed it again, and I always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper. . . . I wished, however, to have had a nicer balance for the purpose: the result was such as to afford a strong presumption that the air was reconverted into water, and *therefore that the origin of it had been air.*"

These passages, when compared with the accounts given of his own work by Cavendish, strikingly exemplify the difference in the character of the two experimentalists. It would be difficult to pack a greater number of blunders into a couple of paragraphs than are contained in these sentences. The expressions in italics show that Priestley wholly failed to comprehend the true origin of the water. In his laudable anxiety to free the two gases from extraneous moisture he committed blunder after blunder. His method of obtaining the oxygen was bad; that of procuring the inflammable air was worse. Both the gases must have been highly impure, and it was a physical impossibility that they should have given their aggregate weight in

water, even after making every allowance for Priestley's crude and imperfect method of determining it.

Bad as the experimental work was, what it appeared to teach was not lost on Watt: it clearly proved to him that water and air were mutually convertible. How the theory took shape in his mind is evident from the terms in which the two series of Priestley's experiments are coupled together in his letters to Gilbert Hamilton, to Deluc, and to Black. Each set is regarded as complementary to the other, and both taken together are held to prove that air and water are mutually convertible and are therefore essentially the same. Under date 21st April 1783, he tells Black that "Dr. Priestley has made many more experiments on the conversion of water into air, and I believe I have found out the cause of it; which I have put in the form of a letter to him which will be read at the Royal Society with his paper on the subject." He then proceeds to give Black a summary of the three sets of facts, or supposed facts, on which he bases his generalisation, and he makes use of these significant words: "In the deflagration of the inflammable and dephlogisticated airs, the airs unite with violence, become red hot, and, on cooling, totally disappear. The only fixed matter which remains is *water*; and *water*, *light*, and *heat* are all the products. Are we not then authorised to conclude that water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat, and that dephlogisticated, or pure air, is composed of water deprived of its phlogiston and united to heat and light; and if light be only a modification of heat, or a component part of phlogiston, then pure air consists of water deprived of its phlogiston and of latent heat."

Very similar turns of expression and trains of reasoning are to be met with in other letters to his friends written at about the same period. In all, it is abundantly clear that, whatever may have been his surmises as to the real nature of water, it was the conception of the mutual convertibility of air and water that was uppermost in his mind. These passages, however, constitute Watt's claim to be regarded as the true and first discoverer of the compound nature of water.

Three days after the letter to the Royal Society was written, or rather dated, there came a bolt from the blue in the form of a letter from Priestley to Watt. "Behold," it said, "with surprise and with indignation the figure of an apparatus that has utterly ruined your beautiful hypothesis, and has rendered some weeks of my labour in working, thinking, and writing almost useless." The doubts of Wedgwood, certainly no mean authority on the properties of baked clay, had, in fact, led Priestley to devise an experiment by which it was proved beyond all doubt that this seeming conversion of water into air was really due to an interchange of steam and air, effected by diffusion through the porous material of the retort. Well might Priestley cry to Deluc, "We are undone!" Watt's faith in the "beautiful hypothesis" was no doubt rudely shaken, but it was not shattered. In his answer to Priestley he denied that it was ruined: "It is not founded," said he, "on so brittle a basis as an earthen retort." Priestley, however, would have none of it; theories with him—always excepting the all-comprehensive one of phlogiston, which was the head and front of his creed, as, indeed, of his subsequent offending—had at no time much value, for, as Marat said of

Lavoisier, he abandoned them as readily as he adopted them, changing his systems as he did his shoes. Indeed, he rather prided himself on this capacity for quick change. "We are, at all ages," he once said, "but too much in haste to *understand*, as we think, the appearances that present themselves to us. If we could content ourselves with the bare knowledge of new facts, and suspend our judgment with respect to their causes, till by their analogy we were led to the discovery of more facts of a similar nature, we should be in a much surer way to the attainment of real knowledge." With a candour all his own, he immediately added: "I do not pretend to be perfectly innocent in this respect myself, but I think I have as little to reproach myself with on this head as most of my brethren; and whenever I have drawn general conclusions too soon, I have been very ready to abandon them. . . . I have also repeatedly cautioned my readers, and I cannot too much inculcate the caution, that they are to consider new *facts* only as discoveries, and mere *deductions* from these facts as of no kind of authority; but to draw all conclusions, and form all hypotheses, for themselves."

Watt's mind was of a very different cast. He did not lightly adopt opinions; his convictions were slowly and deliberately formed, and were retained with a corresponding tenacity. But, all the same, he eventually thought it prudent to withdraw his letter, and three days prior to the reading of Priestley's paper which accompanied it, Priestley informed Sir Joseph Banks of Watt's desire that the letter should not be publicly read. That it was withdrawn on account of what Watt calls Priestley's "ugly experiment," is stated by him in a letter to Black, on the ground that

this experiment rendered "the theory useless, in so far as relates to the change of water into air. . . . I have not given up my theory [that is as to the mutual convertibility of water and air], though neither it, nor any other known one will account for this experiment."

In the meantime, Cavendish had been pursuing his inquiries, and towards the end of this year (1783) he was prepared to give the explanation of the cause of the disturbing factor in his proof of the real nature of water, that is, the origin of the occasional and apparently haphazard presence of small quantities of nitric acid. This he demonstrated to be due to the difficulty of excluding a greater or less quantity of atmospheric nitrogen from the gases employed, and he determined the conditions under which this nitrogen led to the formation of the acid, the true nature of which he thus for the first time established. The account of his labours was read to the Royal Society on 15th January 1784.

In the previous autumn, however, disquieting rumours reached this country that the French philosophers, and chief among them Lavoisier, were poaching upon the English preserves. This circumstance is alluded to in a letter from Watt to Deluc, dated November 30, 1783. "I was at Dr. Priestley's last night. He thinks, as I do, that Mr. Lavoisier, having heard some imperfect account of the paper I wrote in the spring, has run away with the idea and made up a memoir hastily, without any satisfactory proofs. . . . I, therefore, put the query to you of the propriety of sending my letter to pass through their hands to be printed; for even if this theory is Mr. Lavoisier's own, I am vain enough to think that he may get some hints

from my letter, which may enable him to make experiments and to improve his theory, and produce a memoir to the Academy before my letter can be printed, which may be so much superior as to eclipse my poor performance, and sink it into utter oblivion; nay, worse, I may be condemned as a plagiarist, for I certainly cannot be heard in opposition to an Academician and a Financier. . . . But after all, I may be doing Mr. Lavoisier injustice."

That Lavoisier did get some hints, and possibly even through the medium of Watt's letter, is beyond all question. The fact that he was informed of Cavendish's work is specifically stated in Cavendish's memoir, in a passage interpolated by Blagden, the secretary of the Royal Society, and Cavendish's assistant and amanuensis, who himself had told Lavoisier. The whole of the circumstances are set out in detail in a subsequent letter which Blagden addressed to the editor of the *Chemische Annalen* in 1786. That it was known to be Cavendish's experiment that was being thus repeated is confirmed by a letter from Laplace to Deluc, dated June 28, 1783, in which we read, "Nous avons répété, ces jours derniers, Mr. Lavoisier et moi, devant Mr. Blagden, et plusieurs autres personnes, l'expérience de Mr. Cavendish sur la conversion en eau des airs dephlogistiqué et inflammable, par leur combustion. . . . Nous avons obtenu de cette manière plus de $2\frac{1}{2}$ gros d'eau pure, ou au moins qui n'avoit aucun caractère d'acidité, et qui étoit insipide au goût; mais nous ne savons pas encore, si cette quantité d'eau représente le poids des airs consumés; c'est une expérience à recommencer avec toute l'attention possible, et qui me paroît de la plus grande importance." The phrase "qui n'avoit aucun caractère

d'acidité" is of special significance. The French philosophers, and Lavoisier in particular, could with difficulty, as Blagden relates, be brought to credit the statement that when inflammable air was burnt water only was formed: their preconceptions concerning the part played by oxygen in such a case led them to suppose that an acid would be produced. Cavendish was familiar with Lavoisier's doctrine, which is connoted in the very word oxygen, which we owe to the French chemists, and it may be that this circumstance, amongst others, was one cause of the pains he took to understand the origin of the acid he occasionally met with. Lavoisier was led to repeat Cavendish's experiment on 24th June 1783, and on the following day he announced to the Academy that by "the combustion of inflammable air with oxygen very pure water" was formed. It is this statement that has been said to constitute Lavoisier's claim to be considered as the true and first discoverer of the composition of water. That he has no valid claim has been implicitly admitted by Lavoisier himself. The eminent Perpetual Secretary of the French Academy, M. Berthelot, is no doubt accurate in regarding the 25th June 1783 as the first certain date of publication of the discovery that can be established by authentic, *i.e.* official, documents, but as I have elsewhere attempted to show, the circumstances under which that priority of publication was secured give Lavoisier no moral right to the title of the discoverer.¹

Shortly after the reading of Cavendish's memoir to the Royal Society (January 15, 1784), Deluc wrote to Watt, giving an account of its contents, and insinuating

¹ Priestley, Cavendish, Lavoisier, and *La Révolution Chimique*: the Presidential Address to the Chemical Section of the British Association, 1890: see also p. 151 of the present volume.

that its conclusions had been formed in the light of knowledge obtained from Watt's letter to the Royal Society, which although, as we have seen, not publicly read, had, there is no doubt, been perused by others than Priestley, to whom it was originally addressed. Deluc was, no doubt, a zealous friend, but in this matter his zeal outran his discretion. The letter was indeed unworthy of him. He hastens to exculpate Lavoisier and Laplace, but he makes a charge against the honour and integrity of Cavendish for which there was absolutely no justification. He stirs up Watt's suspicions, and then seeks to appease them ; he rouses his anger, and then counsels him to silence by an argument which shows how wholly he misunderstood Watt. Watt's reply was characteristic : " On the slight glance I have been able to give your extract of the paper, I think his theory very different from mine ; which of the two is the right I cannot say : his is more likely to be so, as he has made many more experiments, and consequently has more facts to argue upon.

" As to what you say of making myself *des jaloux*, that idea would weigh little ; for were I convinced I had had foul play, if I did not assert my right, it would either be from a contempt of the modicum of reputation which could result from such a theory ; from the conviction in my own mind that I was their superior ; or from an indolence that makes it easier to me to bear wrongs than to seek redress. In point of interest, in so far as connected with money, that would be no bar ; for though I am dependent on the favour of the public, I am not on Mr. C. and his friends ; and could despise the united power of *the illustrious house of Cavendish*, as Mr. Fox calls them.

" You may, perhaps, be surprised to find so much

pride in my character. It does not seem very compatible with the diffidence that attends my conduct in general. I am diffident, because I am seldom certain that I am in the right and because I pay respect to the opinion of others, where I think they may merit it. At present *je me sens un peu blessé*; it seems hard that in the first attempt I have made to lay anything before the public, I should be thus anticipated."

There was no desire on the part of anybody connected with the management of the Royal Society to withhold from Watt his just due, and it was eventually arranged that his letter to Priestley, together with one he subsequently addressed to Deluc, should be publicly read to the Fellows, and they were subsequently ordered to be printed in the *Philosophical Transactions* in such manner as their author might desire. By his directions, the two letters were merged together, and they appear as having been read on April 29, 1784, under the title, "*Thoughts on the Constituent Parts of Water, and of Dephlogisticated Air: with an account of some experiments on that Subject. In a letter from Mr. James Watt, Engineer, to Mr. Deluc, F.R.S.*" The greater part of the "thoughts" are concerned with the dephlogisticated air; what relate to water have already been given in the extracts from his correspondence. The terms of the letter to Deluc, as printed in the *Philosophical Transactions*, are substantially identical with those of the letters to Black, Hamilton, Smeaton, and Fry.

I have now given all the essential facts which led to the recognition of the true chemical nature of water, and I have stated as accurately and as impartially as I could the relative shares of Watt, Cavendish, and Lavoisier in their discovery and interpretation. As regards Lavoisier, it cannot be claimed that he was the first to obtain the

facts. To Cavendish belongs the merit of having first supplied the true experimental basis upon which accurate knowledge could alone be founded. Watt, on the other hand, although reasoning from imperfect and indeed altogether erroneous data, was the first, so far as we can prove from documentary evidence, to state distinctly that water is not an element, but is composed, weight for weight, of two other substances, one of which he regarded as phlogiston and the other as dephlogisticated air. It would be a mistake, however, to suppose that Watt taught precisely the same doctrine of the true nature of water that we hold to-day. Nor did Cavendish utter a more certain sound. What we regard to-day as the expression of the truth, we owe to Lavoisier, who stated it with a directness and a precision that ultimately swept all doubt and hesitation aside—except to the mind of Priestley, whose “random experiment” gave the first glimmer of the truth.

In this respect the conclusion of Lord Brougham is most just. It was a reluctance to give up the doctrine of phlogiston, a kind of timidity on the score of that long-established and deeply-rooted opinion, that prevented both Watt and Cavendish from doing full justice to their own theory; while Lavoisier, who had entirely shaken off these trammels, first presented the new doctrine in its entire perfection and consistency.

We thus see that each of these eminent men took an independent and, we may say, an equally important share in the establishment of one of the greatest scientific truths that the eighteenth century brought to light.

As regards Watt, the history of this incident serves to bring out only more clearly what we know to be the true character of the man. It illustrates the vigour of his intellectual grasp, the keenness of his mental vision.

At the same time, it exhibits his love of truth for truth's sake ; his unaffected modesty, and the sense of humility that was not the less real because accompanied by a sense of what his inherent love of rectitude taught was due also to himself. The voice of envy and detraction has not been unheard amongst the strife of partisans in the Water Controversy, but throughout it no syllable has been breathed that reflected even remotely upon his honour and integrity.

VI

ANTOINE-LAURENT LAVOISIER

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“IL a été assez heureux ou assez sage, pour que l'on ne sache presque autre chose de lui, et qu'il n'y ait dans son histoire d'autre incidens que des découvertes.” These words were spoken by Cuvier, the Perpetual Secretary of the French Academy, on the occasion of his *éloge* on Cavendish, the discoverer of the compound nature of water, who, in his old age, had been elected a member of the Institute. At first sight they may seem a mere paraphrase of a saying which has become almost trite, but to those who heard them for the first time they had a significance which must have been realised with something like a pang. For at such a time, not one of Cuvier's hearers could have been unmindful of 1794, or have been unmoved by the recollection of a tragedy in which the most illustrious of Cavendish's contemporaries, a man whose life had been dedicated to the cause of humanity, and whose services to science have reflected an imperishable lustre upon France, was sacrificed to the blind fury of his countrymen. Indeed, to the lively and sympathetic intelligence of such an auditory, quickened as it must have been by the singular charm of the speaker's style, his profound sensibility, and rhetorical skill, the strong dramatic element in the

situation could hardly have remained unperceived. Lavoisier and Cavendish were, in a sense, national types ; they were, too, when at the summit of their intellectual power, the acknowledged representatives of two opposing schools of thought. Both were aristocrats, and both, from being poor, became very rich : Cavendish, indeed, was, as M. Biot has said, “le plus riche de tous les savans et probablement aussi le plus savant de tous les riches.” But here the resemblance ends : in character, temperament, and genius, in everything that constitutes individuality, the men were as wide asunder as the poles. Cavendish has been described by his biographer Wilson as the most passively selfish of mortals—a sort of scientific anchorite, who maintained, during the four-score years of his existence, a rigid, undeviating indifference to the affairs of his fellow-men. This embodiment of a clear, cold, passionless intelligence was dead to every æsthetic sense, and had no element of anything that was enthusiastic or chivalrous in its composition. To Cavendish science was, in truth, measurement. “His Theory of the Universe,” says Wilson, “seems to have been that it consisted *solely* of a multitude of objects which could be weighed, numbered, and measured ; and the vocation to which he considered himself called was to weigh, number, and measure as many of these objects as his allotted threescore years and ten would permit. He weighed the Earth ; he analysed the Air ; he discovered the compound nature of Water ; he noted with numerical precision the obscure actions of the ancient element, Fire.” But all this work was done primarily for himself, and to satisfy the questionings of his own intelligence. To give the results of it to the world was hardly a part of his plan, for he cared nothing for the world, and was absolutely indifferent to the interests or

judgment of his fellows. And yet Cavendish was revered, even if he was not loved, during his long and uneventful life, and at his death was laid to rest with every mark of honour and respect in the splendid tomb which his ancestress, Elizabeth Hardwicke, had built for herself and her descendants.

On the other hand, Lavoisier was a man in whom the elements were kindly mixed. No one could more truly say of himself, "Homo sum : humani nihil a me alienum puto." He was ardent, enthusiastic, fond of the society of his fellows, a man of generous impulses, of catholic tastes, and of lofty aims. As a philosopher his influence throughout Europe was supreme, and the manner in which his renown was won was of a kind to strike the national imagination and to minister to the national pride. At the zenith of his fame he was as much a Dictator in the world of science as Napoleon became in the world of politics. But in the very plenitude of this power he was struck at by Fouquier-Tinville, and he who had laboured unceasingly for the glory and well-being of his country was declared guilty of complicity in a conspiracy "against the French people tending to favour by all possible means the success of the enemies of France." Lavoisier's crime was that he had been a *Fermier-général*. He was accused, in the words of the indictment, "of adding to tobacco water and other ingredients detrimental to the health of the citizens." It was a feeble enough weapon to throw even at a *Fermier-général*, but it was thrown with terrible effect. Even to be suspected of tampering with the tobacco of a "citizen" sufficed for the tribunal before which he was brought, although it taxed the ingenuity of Liendon to show how this alleged sophistication brought the accused within the same section of the penal code that swept the

Dantonists to the scaffold. Coffinhal, the Vice-President of the Tribunal, pronounced the judgment: "The Republic has no need of men of science," and within twenty-four hours the tumbrel was on its way to the Place de la Révolution, and, as the *procès-verbal* sets forth, "sur un échafaud dressé sur la dite place, le dit Lavoisier, en notre présence, subi la peine de mort." Well might Lagrange say to Delambre: "It required but a moment to strike off this head, and probably a hundred years will not suffice to reproduce such another."¹

The main events in the scientific career of the great French chemist are tolerably well known, and his position in the history of the development of chemistry is now fully assured. The story of his scientific life has recently been told by M. Berthelot with all the charm and tact which characterise the *éloges* which it is the duty of the secretaries of the Academy from time to time to prepare. Although English men of science may think that M. Berthelot occasionally fails to mete out the strict justice to their countrymen that historical accuracy demands, there cannot be a doubt, in spite of all legitimate deductions, that Lavoisier remains the dominant figure in the chemical world of the eighteenth century. There is much, however, in his life and work, and especially in the circumstances which led to his untimely death, on which we would gladly have more information. Amongst the crop of literature which the centenary of the Revolution has brought forth in France, the historian of science has welcomed, therefore, with special interest,

¹ The Republic, a few months afterwards, found also that it had no need of Coffinhal: he fell with Robespierre, and was guillotined on the 18th Thermidor of the year II. Fouquier-Tinville and some half-dozen others who had been concerned in the trial of Lavoisier were also brought to the scaffold at about the same time.

the admirable monograph on Lavoisier which we owe to the patient industry and patriotic zeal of Professor Grimaux.¹

It must have struck many people, as M. Grimaux tells us it has struck him, that, in spite of the glory which surrounds the name of Lavoisier, it is remarkable that the life of the creator of modern chemistry had still to be written. Beyond the short biographical notices by Lalande, Fourcroy, and Cuvier, which deal mainly with Lavoisier as a man of science, we know practically nothing of the story of a life which was wholly devoted to the public good. Even the world of science knows scarcely anything of his private life, of his virtues, of his intelligent philanthropy, and of the services which he rendered to his country as an academician, an economist, an agriculturist, and a financier. Luckily for his biographer, Lavoisier was a man of perfect method, and he preserved all his manuscripts without exception. After his death these papers were religiously guarded by Madame Lavoisier, from whom they passed to Madame Léon de Chazelles, her grandniece. This, together with other material preserved at the Château de la Canière, where also are kept Lavoisier's books and instruments, has served M. Grimaux as the basis of his book. In addition, he has searched through the public archives, with the result that we have now presented to us for the first time the details of Lavoisier's political life and the true story of his trial and condemnation.

Antoine-Laurent Lavoisier was born in Paris on 26th August 1743. His father, Jean-Antoine, was an advocate; his mother, *née* Punctis, died when he

¹ *Lavoisier*, 1743-1794. "D'après sa correspondance, ses manuscrits, ses papiers de famille, et d'autres documents inédits." Par Edouard Grimaux. Paris: Félix Alcan, 1888.

was five years old, and he and a young sister, who lived only a few years, were taken charge of by the grandmother and her daughter, Mdlle. Constance Punctis. The family was rich, and was able to send the young Antoine to the Collège Mazarin, where he seems to have acquired that passion for natural science which was the motive power of his life. He studied mathematics with the Abbé La Caille, well known for his measurement of an arc of the meridian at the Cape of Good Hope, and for his determination of the length of a seconds pendulum; he learnt botany from Bernard de Jussieu, and geology and mineralogy from Guettard. But it was principally by Rouelle's teaching that the particular direction of Lavoisier's scientific activity was shaped. Guillaume-François Rouelle is mainly remembered by chemists to-day as having just missed the discovery of the Law of Combination by Definite Proportions. By his contemporaries he was considered to have said more "good things" than any man of his time. In spite of his oddities, he exercised an extraordinary influence as a teacher; his lecture-room at the Jardin du Roi was crowded by auditors from all parts of Europe, and among his pupils were Macquer, Bucquet, d'Arcet, and Lavoisier, the men who were destined to make the end of the eighteenth century one of the most stirring epochs in the history of chemistry.

Lavoisier was originally intended for the profession of the law, and actually became a licentiate in 1764, but at the instigation of Guettard, whom he accompanied in his journeys through France, and to whom he was of assistance in the preparation of his great Mineralogical Atlas, he abandoned that career and gave himself up to science. In 1765 he sent his first

paper to the Academy—a modest enough communication on gypsum, but noteworthy as giving for the first time the true explanation of the setting of plaster of Paris, and of the reason that overburnt gypsum will not rehydrate.

In the following year he was awarded a medal by the Academy for an essay on the lighting of large towns, and in the same year he was placed upon the list of candidates for election into that august body. The Académie des Sciences has suffered frequent internal changes, but in the middle of the eighteenth century it was subject to the constitution of 1699, as modified in 1716. It was composed of members of very different orders, enjoying very unequal rights. There were twelve honorary members chosen from among the great nobles, and from whom were selected the president and vice-presidents; eighteen pensionaries, twelve associates, and twelve *adjoints* distributed among the geometers, astronomers, mechanicians, chemists, and botanists; in addition there were a number of free associates, superannuated associates, and pensionaries. The honorary members and the pensionaries had alone a deliberative voice in the elections, and in the business of the Academy. The two associates in the class in which there was a vacancy were, however, called upon, in company with three pensionaries, to draw up the list of candidates. The *adjoints* had practically no privileges beyond that of sitting next to an associate when there was room; at other times they sat upon the benches placed behind the arm-chairs of the associates.

The 18th of May 1768, when the young Lavoisier gained his seat upon the back benches, was a red-letter day in the history of the house of Punctis. It was

no less important in the history of the Academy, for the young *adjoint* was destined to be its champion and do battle for its existence during the dark and terrible time of the Revolution. Lavoisier's extraordinary power of work, his intellectual keenness, and range of knowledge, were quickly recognised, and in spite of his youth he was charged with the preparation of numerous Reports. This part of an academician's duty was as difficult and irksome as it was delicate. During the twenty-five years of his connection with the Academy he contributed upwards of two hundred reports on such disconnected matters as the theory of colours, the areometer of Cartier, modes of determining longitudes, arm-chairs for invalids, prison reform, water supply, the cold of the winter of 1776, the pretensions of Mesmer, the aerostatic inventions of Montgolfier, the imposture of the divining-rod, etc., etc.

Almost immediately after Lavoisier had thus planted his foot on the ladder of fame, he set it unconsciously on the first step of the scaffold. *Adjoint* of the Académie des Sciences, he now became *adjoint* of the *Ferme-général*. His friends, the academicians, looked somewhat askance at this action. Lalande tells us that in his election they had been influenced by the consideration that a young man of parts and activity, whose private means placed him beyond the necessity of seeking another profession, would naturally be useful to science, and they now feared that the new duties would clash with what they imagined was to be the real work of his life. But, luckily, there are always some to offer consolation. "Tant mieux!" said the geometer Fontaine, "the dinners that he will give us will be all the better." Although Lavoisier had inherited his mother's fortune, it was hardly sufficient for the career

which he now planned for himself, and by the advice of a friend of the family, M. de La Galaizière, he became *adjoint* of the *Fermier-général* Baudon, in return for a third of his interest in the lease of Alaterre.

But there were doubtless other reasons for the disapproval of the Academy. The *Ferme-général* was a part of the rotten financial system which ultimately landed France in disaster. It was a company of financiers, to whom the State conceded, for a fixed annual sum, payable in advance, the right of collecting the indirect taxes of the country. Created originally by Colbert, its constitution and functions were modified by successive finance-ministers during the reigns of Louis XIV. and Louis XV., as the will of the King, or the exigencies of the national Exchequer determined. At the time that Lavoisier entered it, the number of the *Fermiers-généraux* was sixty, and the sum to be paid in advance for the lease of six years was 90,000,000 livres, together with a *douceur* of 300,000 livres for the Controller-General. The *Fermiers-généraux* received sums on account during the continuance of the lease, but the actual result of the speculation was known only at its expiration. They had to bear all the expenses of management and collection, to enforce the customs and excise regulations, and their profits were subject to all sorts of rebates, perquisites, pensions, and *pots-de-vin*. It need hardly be said that in the time of the Grand Monarch and his worthy great-grandson, the *Ferme* was a very hot-bed of jobbery, corruption, and malversation. There existed no public audit; none, indeed, was possible. Even the Finance Minister could gain but little information of the details of its monetary transactions. In 1774, Terray, towards the conclusion of the first

lease in which Lavoisier was interested, addressed a confidential inquiry to the *Fermiers-généraux* as to the number of beneficiaries which the will of the Court, i.e. the king or his mistresses, had imposed upon the *Ferme-général*. Through the indiscretion of a clerk the list was made public. Paris was scandalised to learn that the pensions alone amounted to upwards of 400,000 livres annually. In addition, the king secured a sixtieth share of the property of the *Ferme*; his sisters and aunts disposed of 50,000 livres; the nurse of the Duke of Burgundy, 10,000 livres; Madame du Barry's physician, 10,000 livres; the Abbé Voisenon, 3000 livres; a court singer, 2000 livres; and so on. Altogether, the Court and its creatures netted in this way fourteen-sixtieths of the proceeds of the lease of Alaterre. Many of the *Fermiers-généraux* themselves outraged public opinion by their prodigality and the luxury of their hotels and *petites-maisons*. The organisation was detested throughout the length and breadth of France. The peasants, too far from the capital to hear the curses which Mercier flung at the Hôtel des Fermes, were constantly witness of the hardships it inflicted, and the terrible retribution which followed any contravention of its decrees. The taxes were most unequally levied; each province had its own frontier, and to a population impoverished and on the verge of starvation there was every temptation to smuggle; conflicts with its officers were of almost daily occurrence; no house was safe against domiciliary visits, and hundreds of persons were yearly sent to the galleys for the most trifling acts of contraband. It is true there was the *Court des aides*, to which the peasant might appeal against the imposition of the *Ferme*, but too frequently he found that the "gratuitous justice" thus dealt out to him meant only

“justice by gratuities.” Nor was it only on the frontiers that smuggling prevailed. It was calculated that at least one-fifth of the merchandise that entered Paris was contraband. To render the collection of the *octroi* more certain, and to check irregularities, the *Ferme* proposed to surround the city with a wall. Public feeling against the project was intense. A wit of the period declared that “le mur murant Paris rend Paris murmurant.” Military opinion also was adverse to the proposal; the Duke de Nivernais, a Marshal of France, is reported to have said that its author deserved hanging from one of his own towers; and Marat subsequently denounced Lavoisier as the originator of what the citizens were taught to regard as an ingenious method of robbing them of the pure air of the country.

There were, of course, honest *Fermiers-généraux*—men like Delahante, Paulze, d’Arlincourt, and others, and Lavoisier was of the number, who discharged their trust honourably, and who sought to introduce order and good management into the affairs of the society. With the advent of the better times which the beginning of the reign of Louis XVI. seemed to promise, and under the administration of Turgot, the character of the *Ferme-général* improved. With each new lease the position and influence of Lavoisier was strengthened, until, in 1783, he was placed by d’Ormesson upon the Committee of Administration, the most important of the whole, and the only one which had direct relations with the Government. He was thus enabled to remedy many abuses, and to mitigate in various ways the burden of imposition under which France groaned. But it was too late. Nothing the *Ferme* could do would ever wipe out the memories of its exactions. With the growth of Lavoisier’s power and influence in the *Ferme*,

the odium with which it was regarded seemed gradually to concentrate itself upon him. His rectitude, his public services, the purity of his private life, the splendour of his scientific achievements, were unheeded. In the day of reckoning he was remembered only as Lavoisier the *Fermier-général*.

M. Grimaux has been at considerable pains to lay the details of Lavoisier's connection with the *Ferme-général* before us. He estimates that, in all, he acquired, from 1768 to 1786, nearly 1,200,000 livres. He continued to be a member of the *Ferme* until it was suppressed by a decree of the National Assembly in 1791, when its liquidation was confided to six of his colleagues.

Lavoisier's success in administration induced Turgot to consult him on the means of ensuring a regular supply of gunpowder for the service of the State. Prior to Turgot's ministry the manufacture of the gunpowder required for the national defence was entrusted to a financial company, with the result that, on more than one occasion, France was obliged to sue for peace from inability to provide herself with the munitions of war. The *Ferme des Poudres* was managed solely in the interest of its members: waste, peculation, and jobbery were as rampant as in the old days of the *Ferme-général*. Turgot changed all this. In 1775 he created the *Régie des Poudres* and nominated four commissioners, who should be directly responsible to the State for the manufacture of gunpowder. Lavoisier is expressly named as one of the commissioners, as being known, not only for his chemical knowledge, so necessary for administrative work of this kind, but also for the diligence, capacity, and honesty with which he discharged his duties as a *Fermier-général*. At his sugges-

tion, Turgot invited the Academy to offer a prize for the best essay on the economical production of saltpetre, with a view of obtaining information on the modes of manufacture practised in various parts of Europe. No detail of administration was too minute to escape his attention. He regulated the conditions under which the *employés* were selected; he simplified the methods of manufacture and refining of saltpetre, and by successive improvements in composition and modes of mixing he greatly increased the ballistic properties of gunpowder. He who was condemned in 1794 as an enemy to his country was in 1780 recognised as having contributed to its triumphs by augmenting the force of its arms. At times the exercise of his duties placed him in considerable danger, as, for example, in the explosion which resulted in the experiments made to introduce Berthollet's newly discovered chlorate of potash in the place of nitre. But no gunpowder-mill under Lavoisier's charge was half so explosive as Paris in 1789. The events of July had demoralised the city, and it was only too ready to give heed to the slanders and coarse invective of the *Père Duchesne* of Marat and of other self-styled "Friends of the People." The air was full of plots and counter-plots. An order to transport some gunpowder was maliciously misconstrued; the report was spread that it was to be given to the enemies of the nation, and Lavoisier and his fellow-commissioner, Le Fauchaux, nearly fell victims to an angry mob which surged round the gates of the arsenal.

Lavoisier's journeyings through France in connection with the work of the Mineralogical Atlas and as a *Fermier-général*, had taught him much concerning the life of the peasant. Indeed, no Frenchman of his time knew his country better, or was more keenly alive to

the vast economic movement which was slowly gathering strength during the latter half of the eighteenth century. His interest in this movement was no doubt quickened by, even if it did not originate in, his connection with the *Ferme*. It was obvious to him that the whole fiscal system of the country fell with the most crushing effect upon the class least able to bear it, and in the numerous commissions in which he took part he repeatedly indicated the economic disadvantages with which the cultivators of the soil had to contend. In 1785 he became a member, and immediately afterwards secretary of the Committee of Agriculture—a consultative body created by Calonne for the purpose of enlightening the controller-general on agronomic matters in general. Lavoisier not only held the pen; he was the directing spirit of the Committee. He drew up reports and instructions on the cultivation of flax, of the potato, on the liming of wheat; he prepared a scheme for the establishment of experimental farms, and for the collection and distribution of agricultural instruments, for the better adjustment of the tithes and of the rights of pasturage, etc. He was no mere theorist in these matters. In 1778, when he acquired the demesne of Fréchaines, the condition of the peasant was wretched in the extreme. Cultivated grazing land was unknown; the farmers, from the impossibility of feeding their cattle during the winter, had but few beasts; the fields were unmanured; and the yield of corn, even in the best years, was only about five times the weight of the seed. With a view of demonstrating how much might be done by improved methods of tillage, he decided to make trials on above 80 hectares of the worst land on the demesne; and he divided about 240 hectares into three farms, of which he directed the cultivation with all the

precision of laboratory trials. He introduced the cultivation of the beetroot and potato, hitherto unknown in the Blésois. He improved the breed of sheep by the importation of rams and ewes from Spain, and that of cows by the introduction of animals from the model farm of Chanteloup. In 1788, when he presented to the Society of Agriculture the results of his ten years' experience, he again set forth the various disadvantages under which the cultivator laboured — short leases, insecurity of tenure, want of capital, and of credit ; and he made a strong appeal to the proprietors to spend more on the amelioration of their land in order to improve the condition of those who were obliged to live upon it. Each succeeding year saw a change for the better in the lot of the peasants at Fréchaines. In 1793 the crop of wheat had doubled itself, and was more than ten times the weight of the seed, and the number of beasts on the estate had increased fivefold. In the following year came the end, but the memory of the man who was a veritable father of his people is still cherished in the district of Blois.

Lavoisier's position as a landed proprietor was doubtless the cause of his selection as a member of the Assembly of the Orléanais, a sort of County Council created in 1787, according to a plan devised by Necker during his first administration. It was composed of twenty-five members selected by the king, six for the clergy, six for the nobility, and twelve for the third estate, together with the Duke of Luxembourg as president. The twenty-five so nominated were directed to elect an equal number of colleagues, the same proportion being observed for the three orders. The duties of the Assembly were to determine the modes of levying the taxes, to undertake the construction and maintenance

of the highways, and to consider how the commerce and industry of the province might best be developed. Lavoisier, although an esquire, was chosen as a member of the third estate, and he at once became the leader of that section. In the town library of Orleans are preserved the minutes of the Provincial Assembly, together with such of the manuscripts of Lavoisier as relate to its business. During the greater part of its existence the Assembly was engaged in attempts to settle the mode of incidence and collection of the taxes. The third estate demanded the abolition of the exemptions enjoyed by the nobles; the substitution of a fixed subscription for the tithes, which fell with especial severity on the smaller proprietors; and the abolition of the *corvée*, which compelled the peasants to undertake the construction and maintenance of the roads. On all these questions Lavoisier was the mouthpiece of his order. He also introduced schemes for the founding of saving and discount banks, workhouses, and insurance societies, for the creation of nursing establishments, for the formation of canals, and for the exploitation of the mineral productions of the province. "Celui qui fait tout, qui anime tout, qui se multiplie en quelque sorte, c'est Lavoisier; son nom reparaît à chaque instant."¹

Few, if any, of these projects were realised. The Provincial Assemblies might initiate, but they were powerless to execute, and in 1790 they became merged into the States-General, to which Lavoisier was sent as *Député suppléant* for the bailiwick of Blois, having for his colleague the unfortunate Vicomte de Beauharnais, whose widow, Josephine Tascher de la Pagerie, became the wife of Bonaparte. In the same year he was elected a member of the Commune of Paris, and of the

¹ Leonce de Lavergne, *Les Assemblées Provinciales*.

famous club of 89, of which he was ultimately secretary. This institution, which sought to develop, defend, and propagate the principles of constitutional liberty, numbered amongst its adherents all who were eminent in art, literature, science, and politics in France. It had, however, but little influence on the main currents of political thought, and absolutely none on the political action of the time; it dealt too largely with questions of political metaphysics to stem the forces which ultimately gained an overwhelming strength. It ended by being suspected of "aristocratism," and it became a crime to have been one of its members. At the beginning of 1794 the Jacobins expelled from their club all who had been part of the Society of 89 as, *ipso facto*, guilty of "incivism."

Dark clouds were now rapidly gathering; the days of the Great Terror were approaching, and Lavoisier found himself menaced on every side. The first attack came from Marat. Marat had sought, at the outset of his career, to make a name in science by publishing a Treatise on Fire, full of the crudest and most ridiculous speculations on the nature of combustion, and which he caused the *Journél de Paris* to announce had been received with approbation by the Academy. The statement was absolutely baseless, and Lavoisier, as director of the Academy, said so in a few disdainful words. Marat now vented his rage on the Academy, and in a miserable pamphlet traduced men like Laplace, Monge, and Cassini, accusing them of malversation, and of spending in disgraceful orgies the sums voted for the study of aerostation. But it was specially on Lavoisier that he concentrated all his venom and rancour. "Lavoisier, the putative father of all the discoveries which are noised abroad, having no ideas of his own,

fastens on those of others ; but, incapable of appreciating them, he abandons them as readily as he adopts them, and changes his systems as he does his shoes ! ”

In his paper, the *Ami du Peuple*, he is even more furious :—

I denounce this Corypheus of the Charlatans, Sieur Lavoisier, son of a land-grabber (*grippe-sol*), chemical apprentice, pupil of the Genevese stock-jobber, fermier-général, régisseur of powder and saltpetre, administrator of the Discount Bank, secretary of the king, member of the Academy of Sciences. . . . Would to heaven that he had been strung to the lamp-post on the 6th of August. The electors of La Culture would then not have to blush for having nominated him.

At the same time, Lavoisier, as *Fermier-général*, was the object of repeated and violent attacks in the journals and in various political clubs. The leaders of the Revolutionary party, who clamoured for the abolition of all State control over the manufacture of war material, denounced his administration at the *Régie des Poudres*, and he was shortly afterward removed by the National Assembly. The king, however, so far intervened in his behalf as to order that he should be allowed to remain in undisturbed possession of his rooms in the Arsenal, where he had established a laboratory, on which he had expended a large portion of his private fortune. He had been appointed a member of the National Treasury in 1791, but in 1793, to the regret of his colleagues, he requested to be relieved of his functions. In truth, the strain of a constant anxiety was beginning to react upon him ; he was becoming weary of the incessant struggle against enemies who were as vindictive as they were unscrupulous, and longed for the peace and quietude which he found only in his laboratory. To have property was, in the eyes of the Revolutionary tribunals, to be

guilty of "incivism"; and "incivism" was a crime against the Republic. Lavoisier told Lalande that he expected to be stripped of everything, but he added he was not too old to work, and he would begin life again as an apothecary.

On quitting the Treasury, he was reappointed to the *Régie des Poudres*, but a few months afterwards he resigned the position, although he engaged to continue his studies on the manufacture of powder, and on the methods for the analysis of nitre. It is possible that he may have had some warning of what followed. Three days after his resignation, a commission of the Assembly suddenly entered the Arsenal, placed the papers under seal, and ordered the removal of the commissioners to La Force. The elder Le Fauchaux, one of Lavoisier's colleagues, enfeebled by age and infirmities, killed himself in despair, and the son was thrown into prison.

But however desirous Lavoisier might have been to relinquish political life, it was impossible for him to escape from the penalties and responsibilities of his position. In 1791 he had been named secretary and treasurer of the famous Commission of Weights and Measures, which had undertaken to realise the long-cherished idea of supplying France and the world with an international system of weights and measures based upon a natural unit. He was not only the administrative officer of the Commission; he contributed to the nomenclature of the system, and took a prominent part in directing the determination of the various physical constants on which the measurements ultimately rested, and especially in the determination of the weight of the unit volume of water, on which the value of the standard of mass was based. The project

had to be carried out under conditions which could not possibly have been more disadvantageous. Its realisation largely depended on the cordial co-operation of other nations, and the work of measurement could only properly be conducted at a time of peace. France was torn and distracted by internal dissensions; her national credit was gone; and she was threatened on all sides. Delambre has left us an account of the extraordinary difficulties and dangers under which the geodetical observations were executed. Lavoisier's work in Paris as treasurer was hardly less onerous or less hazardous. The project was more than once imperilled by the vacillating action of the Convention. The sums voted by the Assembly were not always forthcoming from the Treasury, and Lavoisier was occasionally under the necessity of depending upon his own means, or his private credit, for the money which Méchain required in order to extend the measurement of the arc to Barcelona.

Doubtless, much of the difficulty was due to the attitude of the Convention towards the Academy. In turn with every monarchical institution of the time, the Academy was suspected of "incivism," and its destruction was already being compassed. Lavoisier, who had been named treasurer in succession to Tillet, whose long illness had thrown the financial affairs of the learned body into confusion, now found himself confronted with new troubles. The salaries of the Academicians, many of whom were old men, and in straitened circumstances, were in arrears. Lavoisier was again under the necessity of advancing money from his private purse in certain of the more urgent cases. The Society continued to hold its meetings as usual until the spring of 1792, when an unexpected

motion by Fourcroy revealed to the Academicians the danger in which they stood. Fourcroy demanded that the Academy should expel such of its members as were known for their "incivism." The motion was resisted on the ground that the Academy had no concern with the political opinions of its members: the progress of science was its sole business. Fourcroy insisted on his motion, when the geometer Cousin found the way of escape from a position which it was evident had been most skilfully chosen, by proposing that the question should be submitted to the Minister, who would make such erasures from the list as he thought necessary, whilst the Academy should continue to pursue its more intellectual occupations.

This suggestion was adopted, but Fourcroy was not a man to submit tamely to a rebuff, and the Academy soon felt the effect of his resentment. Although the responsible Ministers of the Government still recognised it as the intellectual centre of France, its enemies within the Convention were unceasing in their efforts to overthrow it. The outlook was gloomy in the extreme. The shadow of its impending doom seemed to hang over its meetings. We find at this time in its minutes no mention of the members present, nor of the discussions in which they engaged. Even during the dark days of 1793, Lavoisier, active, hopeful, and courageous as ever, strained every nerve to maintain the continuity of its work; he was the life and soul of the Society, and the ever-watchful guardian of its interests. Together with Haüy and Borda he laboured incessantly at the work of the Commission. He obtained for Vicq d'Azir 8400 livres for the continuation of his treatise on human and comparative anatomy; Jeurat received 300 livres for the calculations for his new lunar tables;

Berthollet the 100 louis which he required for his work on applied chemistry. Even Sage, one of the most bitter opponents of the new chemistry, and Fourcroy still obtained the money which they needed for the prosecution of their investigations. He exerted all his influence with Ministers, with the administrators of the Directory, and with the commissioners of the Treasury, to induce the Government to fulfil its obligations towards the Academy. The eloquence of Grégoire, and the courage of Lakanal for a time averted the final blow, but the enemies of the Academy eventually found they had a majority in the Convention, and they hastened to make use of it. The painter David pronounced the doom of all the learned societies of France, and on August 8, 1793, the Convention decreed their suppression.

Fourcroy had triumphed ; too timorous to work in the open, he had been the unseen power behind the Convention which had steadily undermined the influence of the Academy, and had at length effected its destruction. Still Lavoisier did not despair. He appealed to the Committee of Public Instruction to allow the members of the Academy to continue their labours, and he entreated that the work of the Commission of Weights and Measures might not be interrupted. True to his trust, he pleaded for those of his colleagues who had been reduced to poverty by the decree of the Convention :—

It is unnecessary to add, citizens, that the continuance of their salaries to those who have earned them is demanded by justice ; there is not an academician who, if he had applied his intelligence and means to other objects, would not have been able to secure a livelihood and a position in society. It is on the public faith that they have followed a career, honourable without doubt, but hardly

lucrative. Many of them are octogenarians and infirm ; several of them have spent their powers and their health in travel and investigations undertaken gratuitously for the Government ; the sense of rectitude of Frenchmen will not allow the nation to disappoint their hopes ; they have at least an absolute right to the pensions decreed in favour of all public functionaries. . . . Citizens, the time presses ; if you allow the men of science who composed the defunct Academy to retire to the country, to take other positions in society, and to devote themselves to lucrative occupations, the organisation of the sciences will be destroyed, and half a century will not suffice to regenerate the order. For the sake of the national honour, in the interests of society, as you regard the good opinion of foreign nations, I beseech you to make some provision against the destruction of the arts which would be the necessary consequence of the annihilation of the sciences.

The Convention was inexorable and Fourcroy relentless. He now acted as if his object was to crush Lavoisier, and by an adroit move he caused him to be stigmatised as a counter-revolutionist. A few days afterwards the Convention ordered the arrest of Lavoisier, together with the rest of the *Fermiers-généraux* who had signed the leases of David, Salzard, and Mager, and he was conducted to the prison of Port-Libre. Every effort on the part of his friends was put forth to save him. The Commission of Weights and Measures, headed by Borda and Haüy, appealed to the Committee of Public Safety. It refused to discuss the petition, and two days afterwards, on the advice of the Committee of Public Instruction, of which Fourcroy and Guyton Morveau were members, the names of Borda, Lavoisier, Laplace, Coulomb, Brisson, and Delambre were removed from the Commission. The Committee of Assignats requested in vain that Lavoisier might be allowed to work in his laboratory. The Bureau de Consultation des Arts et Métiers, of which Lavoisier was president at the time of his

arrest, addressed a memorial extolling the value of his public services, and drawing special attention to the fact that the scheme of national instruction then before the Convention was entirely of his creation. All was in vain. The Terrorists were in complete ascendancy in the Convention. Robespierre had swept Hébert and Danton from his path, and the work of "purification" was going on merrily. On May 8, 1794, the *Fermiers-généraux* were brought to trial, but their condemnation had already been pronounced. Liendon, in the turgid rhetoric of the period, demanded the heads of the prisoners . . . "the measure of the crimes of these vampires is filled to the brim . . . the immorality of these creatures is stamped on public opinion; they are the authors of all the evils that have afflicted France for some time past." Hallé attempted to intervene on behalf of Lavoisier, and presented the memorial of the Bureau de Consultation; Coffinhal, who presided, pushed it aside, with the memorable words: "La République n'a pas besoin de savants; il faut que la justice suive son cours." The twenty-eight *Fermiers-généraux* were found guilty of death. They were sentenced to be executed within twenty-four hours, and it was ordered that their property should be confiscated to the Republic. Such was the haste of the judges that the decision of the jury was omitted from the minute of judgment—an act of informality which Dobsen used with terrible effect a few months later, when Fouquier-Tinville and Coffinhal found themselves in the place of the unfortunate *Fermiers-généraux*.

On the following morning the condemned men were taken from the Conciergerie to the Place de la Révolution. They bade each other farewell; Papillon d'Auteroche, seeing the crowd *en carmagnole* as the

carts passed through the streets, raised a smile as he said disdainfully, in allusion to the confiscation of his effects: "What annoys me is to have such disagreeable heirs." They were guillotined in the order of their names on the indictment. Lavoisier saw fall the head of his father-in-law, and was himself the fourth to suffer. In common with all his companions, he bore himself with dignity, and met his end calmly and with courage. The spectacle of such fortitude awed the crowd into silence, and no reproach or insult reached the ears of the dying man.

Thus perished, at the age of fifty-one, one of the most remarkable men in the history of science. All that was mortal of him was thrown into the cemetery of the Madeleine, and the place of his interment was forgotten. The news of this great crime profoundly affected the intellectual world. There was not a scientific body in Europe that failed to give utterance to its sense of shame and sorrow. With the fall of Robespierre this feeling penetrated France. On October 22, 1795, Lalande pronounced an *éloge* on Lavoisier before the Lyceum of Arts, and in the midst of the extraordinary revulsion of popular feeling which preceded the days of the Directory the same body decreed a solemn funeral ceremony in his honour. It was, in truth, a lugubrious farce, marked by all the extravagances of taste and sentiment which were then in fashion, and it was crowned by an *éloge* . . . from *Fourcroy*! Time-serving and timorous as ever, Fourcroy had no other extenuation than an appeal to the memories of the Great Terror. "Carry yourselves back to that frightful time . . . when terror separated even friends from each other, when it isolated even the members of a family at their very fireside, when the

least word, the slightest mark of solicitude for the unfortunate beings who were preceding you along the road to death, were crimes and conspiracies." For Fourcroy to plead that he was pusillanimous hardly serves to exculpate him. He would have us believe that he was powerless to avert the catastrophe he now affects to deplore; but he stands charged, on his own showing, with participation in acts which largely contributed to it, and the charge rests heavily on his memory.

VII

PRIESTLEY, CAVENDISH, LAVOISIER, AND *LA RÉVOLUTION CHIMIQUE*

THE PRESIDENTIAL ADDRESS TO THE CHEMICAL SECTION OF THE
BRITISH ASSOCIATION, LEEDS, 1890

LEEDS has one most notable association with chemistry of which she is justly proud. In the month of September 1767 Dr. Joseph Priestley took up his abode in this town. The son of a Yorkshire cloth-dresser, he was born in 1733 at Fieldhead, a village about six miles hence. His relatives, who were strict Calvinists, on discovering his fondness for books, sent him to the Academy at Daventry to be trained for the ministry. In spite of his poverty and of certain natural disadvantages of speech and manner, he gradually acquired, more especially by his controversial and theological writings, a considerable influence in Dissenting circles. A pressing invitation and the prospect of one hundred guineas a year induced him to accept an invitation to take charge of the congregation of Mill Hill Chapel here. He was already known to science by his *History of Electricity*, and the effort was made to attach him still more closely to its cause by the offer of an appointment as naturalist to Cook's Second Expedition to the South Seas. But, thanks to the

intervention of some worthy ecclesiastics on the Board of Longitude who had the direction of the business, and who, as Professor Huxley once put it, "possibly feared that a Socinian might undermine that piety which in the days of Commodore Trunnion so strikingly characterised sailors," he was allowed to remain in Leeds, where, as he tells us in his *Memoirs*, he continued six years, "very happy with a liberal, friendly, and harmonious congregation," to whom his services (of which he was not sparing) were very acceptable. "In Leeds," he says, "I had no unreasonable prejudices to contend with, and I had full scope for every kind of exertion."¹

We have every reason to feel grateful to the "worthy ecclesiastics," since their action indirectly occasioned Priestley to turn his attention to chemistry. The accident of living near a brewery led him to study the properties of "fixed air," or carbonic acid, which is abundantly formed in the process of fermentation, and which at that time was the only gas whose separate and independent existence had been definitely established. From this happy accident sprang that extraordinary succession of discoveries which earned for their author the title of the Father of Pneumatic Chemistry, and which were destined to completely change the aspect of chemical theory and to give it a new and unexpected development.

I have been led to make this allusion to Priestley, not so much on account of his connection with this place as for the reason that, as it seems to me, there has been a disposition to obscure his true relation to the

¹ Leeds still enjoys one of the fruits of Priestley's insatiable power of work in her admirable Proprietary Library. He seems to have suggested its formation, and was its first honorary secretary.

marvellous development of chemical science which made the close of the eighteenth century memorable in the history of learning. Our distinguished fellow-worker, M. Berthelot, the Perpetual Secretary of the French Academy, has recently published, under the title of *La Révolution Chimique*, a remarkable book, written with great skill, and with all the charm of style and perspicacity which invariably characterises his work, in which he claims for Lavoisier a participation in discoveries which we count among the chief scientific glories of this country. From the eminence of M. Berthelot's position in the world of science his book is certain to receive in his own country the attention which it merits, and as it is issued as one of the volumes of the Bibliothèque Scientifique Internationale it will probably obtain through the medium of translations a still wider circulation. I trust that I shall not be accused of being unduly actuated by what Mr. Herbert Spencer terms "the bias of patriotism," in deeming the present a fitting occasion on which to bring these claims to your notice with a view of determining how far they can be substantiated.

All who are in the least degree familiar with the history of chemical science during the last hundred years, will recognise, as I proceed, that the claims which M. Berthelot asserts on behalf of his illustrious predecessor are not put forward for the first time. Explicitly made, in fact, by Lavoisier himself, they were uniformly and consistently disallowed by his contemporaries. M. Berthelot now seeks to support them by additional evidence and to strengthen them with new arguments, and asks us thereby to clear the memory of Lavoisier from certain grave charges which lie heavily on it. You have doubtless anticipated that these claims

have reference to Lavoisier's position in relation to the discovery of oxygen gas and the determination of the non-elementary nature of water.

The substance we now call oxygen—a name we owe to Lavoisier—was discovered by Priestley on August 1, 1774; he obtained it, as every schoolboy knows, by the action of heat upon the red oxide of mercury. We all remember the characteristically ingenuous account which Priestley gives of the origin of his discovery. M. Berthelot sees in it merely the evidence of the essentially empirical character of his work. "Priestley," he says, "the enemy of all theory and of every hypothesis, draws no general conclusion from his beautiful discoveries, which he is pleased, moreover, not without affectation, to attribute to chance. He describes them in the current phraseology of the period with an admixture of peculiar and incoherent ideas, and he remained obstinately attached to the theory of phlogiston up to his death, which occurred in 1804" (p. 40).

Such statements are calculated to give an erroneous idea of Priestley's merit as a philosopher. That the implication contained in the passage is wholly opposed to the real spirit of his work might be readily shown by numerous quotations from his writings. Perhaps this will suffice: "It is always our endeavour, after making experiments, to generalise the conclusions we draw from them, and by this means to form a *theory* or system of principles to which all the *facts* may be reduced, and by means of which we may be able to foretell the result of future experiments." This quotation is taken from the concluding chapter of his *Experiments and Observations on Different Kinds of Air*, in which he actually seeks to draw "general conclusions" concerning the constituent principles of

the various gases which he himself made known to us, and to show the bearing of these conclusions on the doctrine of phlogiston. That he was content to rest in the faith of Stahl's great generalisation, even to the end, is true, and the fact is the more remarkable when we recall the absolute sincerity of the man, his extraordinary receptivity, and, as he says of himself, his proneness "to embrace what is generally called the heterodox side of almost every question." If it is argued that this merely shows Priestley's inability to appreciate theory, it must be at least admitted that there is no proof that he was inimical to it. His position is clearly evident from the concluding words of the section of his work from which I have already quoted: "This doctrine of the composition and decomposition of water has been made the basis of an entirely new system of chemistry, and a new set of terms has been invented and appropriated to it. It must be acknowledged that substances possessed of very different properties may, as I have said, be composed of the same elements in different proportions and different modes of combination. It cannot, therefore, be said to be absolutely *impossible* but that water may be composed of these two elements or any other. But then the supposition should not be admitted without *proof*; and if a former theory will sufficiently account for all the *facts*, there is no occasion to have recourse to a new one, attended with no peculiar advantage (*loc. cit.* p. 543). . . . I should not feel much reluctance to adopt the *new doctrine*, provided any new and stronger evidence be produced for it. But though I have given all the attention that I can to the experiments of M. Lavoisier, etc., I think that they admit of the easiest explanation on the *old system*" (*loc. cit.* p. 563).

The fact that Priestley was the first to consciously isolate oxygen is not contested by M. Berthelot, although he is careful to point out, what is not denied, that the exact date of the discovery depends on Priestley's own statement, and that his first publication of it was made in a work published in London in 1775. It was known before Priestley's famous experiment that the red oxide of mercury, originally formed by heating the metal in contact with air, would again yield mercury by the simple action of heat and without the intervention of any reducing agent. Bayen, six months before the date of Priestley's discovery, had noted that a gas "was thus disengaged, but he gave no description of its nature, contenting himself merely by pointing out the analogy which his observations appeared to possess to those of Lavoisier on the existence of an elastic fluid in certain substances. Afterwards, when the facts were established, Bayen drew attention to his earlier experiments and claimed, not only the discovery of oxygen, but all that Lavoisier deduced from it. "But," says M. Berthelot, in reference to this circumstance, "his contemporaries paid little heed to his pretensions, nor will posterity pay more" (*La Révolution Chimique*, p. 60).

M. Berthelot, however, does not dismiss Lavoisier's claims to a participation in the discovery in the same summary fashion. On the contrary, whilst not explicitly claiming for him the actual isolation, in the first instance, of oxygen, the whole tenor of his argument is to palliate, and even to justify, his demand to be regarded as an independent discoverer of the gas. He begins by asserting that Lavoisier had already a presentiment of its existence in 1774, and he quotes, in support of this assumption, an abstract from Lavoisier's memoir, pub-

lished in December 1774, in the *Journal de Physique* of the Abbé Rozier: "This air, deprived of its fixable portion (by metals during calcination), is in some fashion decomposed, and this experiment would seem to afford a method of analysing the fluid which constitutes our atmosphere and of examining the principles of which it is composed. . . . I believe I am in a position to affirm that the air, as pure as it is possible to suppose it, free from moisture and from every foreign substance, far from being a simple body, or element, as is commonly thought, should be placed, on the contrary, . . . in the group of the mixtures, and perhaps even in that of the compounds."

M. Berthelot further asserts that Lavoisier was at this time the first to recognise the true character of air, and he expresses his belief that it is probable that he would himself have succeeded in isolating its constituents if the path of inquiry had been left to him alone. It is no disparagement to Lavoisier's prescience to say that there is nothing in these lines, nor in the memoir which deals with the repetition of Boyle's experiments on the calcination of tin to which they refer, to show that Lavoisier had made any advance beyond the position of Hooke and Mayow. It has been more than once pointed out that the chemists of the seventeenth century understood the true nature of combustion in air much better than their brethren of the last quarter of the eighteenth century. Hooke, in the *Micrographia*, and Mayow, in his *Opera Omnia Medicophysica*, indicated that combustion consists in the union of something with the body which is being burnt; and Mayow, both by experiment and inference, demonstrated in the clearest way the analogy between respiration and combustion, and showed that in both processes one constituent

only of the air is concerned. He distinctly stated that not only is there increase of weight attending the calcination of metals, but that this increase is due to the absorption of the same *spiritus* from the air that is necessary to respiration and combustion. Mayow's experiments are so precise, and his facts so incontestable, that, as Chevreul has said, it is surprising that the truth was not fully recognised until a century after his researches. (*Vide Watts' Dictionary of Chemistry*, by Morley and Muir; art. "Combustion," p. 242.)

It is now necessary to examine Lavoisier's claims rather more closely and in the light of M. Berthelot's book. A *résumé* of his work *On the Calcination of Tin* was given by Lavoisier to the Academy in November 1774, but the complete memoir was not deposited until May 1777. A careful comparison of an abstract of what was stated to the Academy in November 1774, contributed by Lavoisier himself, in December 1774, to the *Journal de Physique* of the Abbé Rozier, makes it evident that very substantial additions were made to the communication before it was finally printed in the *Mémoires de l'Académie des Sciences*. The possibility of this is allowed by M. Berthelot. He says (p. 58): "A summary communication, often given *viva voce* to a learned society, such as the Academy of Sciences of Paris or the Royal Society of London, would immediately call forth verifications, ideas, and new experiments, which would develop the range and even the results of such communication. The original author, when printing his memoir, would in return—and for this he is hardly blamable—embody these additional results and later interpretations. It thus becomes most difficult to assign impartially to each his share in a rapid succession of discoveries" (*loc. cit.* p. 58).

But although, as we shall see, Lavoisier was certainly aware of Priestley's great discovery, no allusion is made to the gas, nor to Priestley's previous work on the other constituent of air, which is printed in the *Philosophical Transactions* for 1772, and for which he was awarded the Copley Medal by the Royal Society. It is simply impossible to believe that Lavoisier could have been uninfluenced by this work. Indeed, we venture to assert that the full and clear recognition of the non-elementary nature of air which he eventually made was based upon it. It is noteworthy that in the early part of his memoir he states his opinion that the addition not only of powdered charcoal, but of any phlogistic substance to a metallic calx is attended with the formation of fixed air. It is certain that at this period he had not only not consciously obtained any gas resembling Priestley's dephlogisticated air from any calx with which he had experimented, but that none of his experiments had afforded him any idea that the gas absorbed during calcination was identical with it.

At Easter 1775 Lavoisier presented a memoir to the Academy "On the Nature of the Principle which Combines with Metals during Calcination." This was "*reçu le 8 août, 1778.*" To the memoir there is a note stating that the first experiments detailed in it were performed more than a year before; those on the red precipitate were made by means of a *burning glass* in the month of November 1774, and were repeated in the spring of 1775 at Montigny in conjunction with M. Trudaine. In this paper Lavoisier first distinctly announces that the principle which unites with metals during their calcination, which increases their weight, and which transforms them into calces, is nothing else "than the purest and most salubrious part of the air; so that if

that air which has been fixed in a metallic combination again becomes free, it reappears in a condition in which it is eminently respirable, and better adapted than the air of the atmosphere to support inflammation and the combustion of substances" (*Œuvres de Lavoisier*, official edition, vol. ii. p. 123). He then describes the method of preparing oxygen by heating the red oxide of mercury, and compares the properties of the gas with those of fixed air. There is, however, no mention of Priestley, nor any reference to his experiments. It can hardly be doubted that in this memoir Lavoisier intended his readers to believe that he was "the true and first discoverer" of the gas which he afterwards named oxygen. This is borne out by certain passages in his subsequent memoir "On the Existence of Air in Nitrous Acid"; "*lu le 20 avril, 1776, remis en décembre 1777.*" He had occasion incidentally to prepare the red oxide of mercury by calcining the nitrate, and says that he obtained from it a large quantity of an air "much purer than common air, in which candles burnt with a much larger, broader, and more brilliant flame, and which in no one of its properties differed from that which I had obtained from the calx of mercury, known as *mercurius precipitatus per se*, and which Mr. Priestley had procured from a great number of substances by treating them with nitric acid."

In another part of this memoir he says that "perhaps, strictly speaking, there is nothing in it of which Mr. Priestley would not be able to claim the original idea; but as the same facts have conducted us to diametrically opposite results, I trust that, if I am reproached for having borrowed my proofs from the works of this celebrated philosopher, my right at least to the conclusions will not be contested." M. Berthelot remarks on the irony of this passage: we may infer from it that

the friends of the English chemist had not been altogether idle. In his memoir "On the Respiration of Animals," read to the Academy in 1777, he again appears to admit the claim of Priestley to at least a share in the discovery: "It is known from Mr. Priestley's and my experiments that *mercurius precipitatus per se* is nothing but a combination," etc. In several subsequent communications Priestley's name is mentioned in very much the same connection, until we come to the classical memoir "On the Nature of the Acids," when it is said: "I shall henceforth designate the dephlogisticated air, or the eminently respirable air . . . by the name of the *acidifying principle*, or, if it is preferred to have the same signification under a Greek word, by that of the '*principe oxygène*.'"

In none of the memoirs after that of Easter 1775 is the claim for participation more than implied; it is made explicitly for the first time in the paper "On a Method of Increasing the Action of Fire," printed in the *Mémoires de l'Académie* for 1782, and in these words: "It will be remembered that at the meeting of Easter 1775 I announced the discovery, which I had made some months before with M. Trudaine,¹ in the laboratory at Montigny, of a new kind of air, up to then absolutely unknown, and which we obtained by the reduction of *mercurius precipitatus per se*. This air, which Mr. Priestley discovered at very nearly the same time as I, and I believe even before me, and which he had procured mainly from the combination of minium and of several other substances with nitric acid, has been named by him *dephlogisticated air*."

In the "Traité Élémentaire de Chimie" the claim for participation is again asserted in these words:

¹ M. Trudaine de Montigny died in 1777.

"This air, which Mr. Priestley, Mr. Scheele, and I discovered at about the same time." . . .

Now there is no question that Lavoisier knew of the existence of oxygen some months before he made the experiments with the burning glass of M. Trudaine at Montigny, *for the simple reason that Priestley had already told him of it*. Priestley left Leeds in 1773 to become the librarian and literary companion of Lord Shelburne, and in the autumn of 1774 he accompanied his lordship to the Continent, and spent the month of October in Paris. Lavoisier was famous for his hospitality; his dinners were celebrated; and Priestley, in common with every foreign *savant* of note who visited Paris at that period, was a welcome guest. What followed is best told in Priestley's own words: "Having made the discovery [of oxygen] some time before I was in Paris, in the year 1774, I mentioned it at the table of Mr. Lavoisier, when most of the philosophical people of the city were present, saying that it was a kind of air in which a candle burnt much better than in common air, but I had not then given it any name. At this all the company, and Mr. and Mrs. Lavoisier as much as any, expressed great surprise. I told them I had gotten it from *precipitate per se* and also from *red lead*. Speaking French very imperfectly, and being little acquainted with the terms of chemistry, I said *plombe rouge*, which was not understood till Mr. Macquer said I must mean *minium*."

In his account of his own work on dephlogisticated air, given in his *Observations*, etc., 1790 edition, he further says, vol. ii. p. 108: "Being in Paris on the October following [the August of 1774], and knowing that there were several very eminent chemists in that place, I did not omit the opportunity, by means of my

friend Mr. Magellan,¹ to get an ounce of *mercurius calcinatus* prepared by Mr. Cadet, of the genuineness of which there could not possibly be any suspicion; and, at the same time, I frequently mentioned my surprise at the kind of air which I had got from this preparation to Mr. Lavoisier, Mr. Le Roy, and several other philosophers, who honoured me with their notice in that city, and who, I daresay, cannot fail to recollect the circumstance."

If any further evidence is required to prove that Lavoisier was not only not "the true and first discoverer" of oxygen, but that he has absolutely no claim to be regarded even as a later and independent discoverer, it is supplied by M. Berthelot himself. Not the least valuable portion of M. Berthelot's book, as an historical work, is that which he devotes to the analysis of the thirteen laboratory journals of Lavoisier, which have been deposited, by the pious care of M. de Chazelles, his heir, in the archives of the Institute. M. Berthelot has given us a synopsis of the contents of almost every page of these journals, with explanatory remarks, and dates when these could be ascertained. As he well says, these journals "are of great interest because they inform us of Lavoisier's methods of work and of the direction of his mind—I mean the successive steps in the evolution of his private thought." On the fly-leaf of the third journal is written, "*du 23 mars, 1774, au 13 février, 1776.*" From p. 30 we glean that Lavoisier visited his friend M. Trudaine at Montigny about ten days after his conversation with Priestley, and repeated the latter's experiments on the marine

¹ Prof. Grimaux (*Lavoisier*, p. 51), says: "Un de ses [Lavoisier's] amis qui habitait Londres, Magalhaens ou Magellan, de la famille du célèbre navigateur, lui envoyait tous les mémoires sur les sciences qui paraissaient en Angleterre et le tenait au courant des découvertes de Priestley."

acid and alkaline airs (hydrochloric acid gas and ammonia). He is again at Montigny some time between February 28 and March 31, 1775, and repeats not only Priestley's experiments on the decomposition of mercuric oxide, presumably by means of M. Trudaine's famous burning glass, but also his observations on the character of the gas. The fly-leaf of the fourth journal informs us that it extends from February 13, 1776, to March 3, 1778. On p. 1 is an account of experiments made February 13, on "*précipité per se de chez M. Baumé,*" in which the disengaged gas is spoken of as "*l'air déphlogistique de M. Prislej*" (*sic*). Such a phrase in a private note-book is absolutely inconsistent with the idea that at this time Lavoisier considered himself as an independent discoverer of the gas. How he came to regard himself as such we need not inquire. Nor is it necessary to occupy your time by any examination of the arguments by which M. Berthelot, with the skill of a practised advocate, would seem to identify himself with the case of his client. We would do him the justice of recognising the difficulty of his position. He seeks to discharge an obligation, of which the acknowledgment has been too long delayed. The Académie des Sciences a year ago awoke to the sense of its debt of gratitude to the memory of the man who had laboured so zealously for its honour, and even for its existence, during the stormy period of which France has just celebrated the centenary, and out of the *éloge* on Lavoisier which M. Berthelot, as Perpetual Secretary, was commissioned to deliver, has grown *La Révolution Chimique*. To write eulogy, however, is not necessarily to write history. We cannot but think that M. Berthelot has been hampered by his position, and that his opinion, or at least the free expression of it, has been

fettered by the conditions under which he has written. We imagine we discern between the lines the consciousness that, to use Brougham's phrase, the brightness of the illustrious career which he eulogises is dimmed with spots which a regard for historical truth will not permit him wholly to ignore.

Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen and the determination of the compound nature of water. M. Berthelot's contention is that not only did Lavoisier effect the overthrow, but he also discovered the facts. In other words, he has not only a claim to a participation in the discovery of oxygen, but he is also "the true and first discoverer" of the non-elementary nature of water. This second claim is directly and explicitly stated. Although it is supported by a certain ingenuity of argument, we venture to think that we shall be able to show it has no greater foundation in reality than the first.

Members of the British Association who are at all familiar with its history, will recall the fact that this is not the first occasion on which the attempt to transfer "those laurels which both time and truth have fixed upon the brow of Cavendish" has had to be resisted. At the Birmingham Meeting of 1839 the Rev. W. Vernon Harcourt, who then presided, devoted a large portion of his address to an able and eloquent vindication of Cavendish's rights. The attack came then as now from the Perpetual Secretary of the French Academy, and the charges were also formulated then, as now, in an *éloge* read before that learned body. The assailant was M. Arago, who did battle, not for his countryman Lavoisier, whose claims are dismissed as "pretensions," but on behalf of James Watt, the great

engineer, who was one of the foreign members of the Institute.

It is not my wish to trouble you at any length with the details of what has come to be known in the history of scientific discovery as the Water Controversy—a controversy which has exercised the minds and pens of Harcourt, Whewell, Peacock, and Brougham in England; of Brewster, Jeffrey, Muirhead, and Wilson in Scotland; of Kopp in Germany; and of Arago and Dumas in France. This controversy, it has been said, takes its place in the history of science side by side with the discussion between Newton and Leibnitz concerning the invention of the Differential Calculus, and that between the friends of Adams and Leverrier in reference to the discovery of the planet Neptune. Up to now it has practically turned upon the relative merits of Cavendish and Watt. M. Berthelot is the first French *savant* of any note who has seriously put forward the claims of Lavoisier, his countryman and predecessor Dumas having deliberately rejected them.

At the risk of wearying you with detail, I am under the necessity of restating the facts in order to make the position clear. Some time before April 18, 1781, Priestley made what he called “a random experiment” for the entertainment of a few philosophical friends. It consisted in exploding a mixture of inflammable air (presumably hydrogen) and common air, contained in a closed glass vessel, by the electric spark, in the manner first practised by Volta in 1776. The experiment was witnessed by Mr. John Warltire, a lecturer on natural philosophy and a friend of Priestley, who had rendered him the signal service of giving him the sample of the mercuric oxide from which he had first obtained oxygen. Warltire drew Priestley’s attention

to the fact that after the explosion the sides of the glass vessel were bedewed with moisture. Neither of the experimenters attached any importance to the circumstance at the time, Priestley being of opinion that the moisture was pre-existent in the gases, as no special pains were taken to dry them. Warltire, however, conceived the notion that the experiment would afford the means of determining whether heat was ponderable or not, and hence he was led to repeat it, firing the mixture in a copper vessel for greater safety. The results of these observations are contained in Priestley's *Experiments and Observations on Air*, vol. v., 1781, App. p. 395.

At this period Cavendish was engaged on a series of experiments "made, as he says, principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed" (Cavendish, *Philosophical Transactions*, 1784, p. 119). On the publication of Priestley's work he repeated Warltire's experiment, for, he says, as it "seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely." The series of experiments which Cavendish was thus induced to make, and which he made with all his wonted skill in quantitative work, led him at some time in the summer of 1781 to the discovery that a mixture of two volumes of the inflammable air from metals (the gas we now call hydrogen) with one volume of the dephlogisticated air of Priestley combine together under the influence of the electric spark, or by burning, to form the same weight of water. If Cavendish had published the results of these observations at or near the time he

obtained them, there would have been no Water Controversy. But in the course of the trials he found that the condensed water was sometimes acid, and the search for the cause of the acidity (which incidentally led to the discovery of the composition of nitric acid) occasioned the delay. The main result that a mixture of two volumes of inflammable air and one volume of dephlogisticated air could be converted into the same weight of water was, however, communicated to Priestley, as he relates in a paper in the *Philosophical Transactions* for 1783. Priestley was at this time interested in an investigation on the seeming convertibility of water into air, and he was led to repeat Cavendish's experiments, some time in March 1783, on what was apparently the converse problem. Priestley, however, made a fatal blunder in the repetition. With the praiseworthy idea of obviating the possibility of any moisture in the gases, he prepared the dephlogisticated air from nitre, and the inflammable air by heating what he calls "perfectly made charcoal" in an earthenware retort. At this period, it must be remembered, there was no sharp distinction between the various kinds of inflammable air: hydrogen, sulphuretted hydrogen, marsh gas and olefiant gas, coal gas, the vapours of ether and turpentine, and the gas from heated charcoal—consisting of a mixture of carbonic oxide, marsh gas, and carbonic acid—were indifferently termed "inflammable air." Priestley attempted to verify Cavendish's conclusion on the identity of the weight of the gases used with that of the water formed; but his method in this respect, as in his choice of the inflammable air, was wholly defective, and could not possibly have given him accurate results. It consisted in wiping out the water from the explosion vessel by

means of a weighed piece of blotting-paper, and determining the increase of weight of the paper. He says, however: "I always found as near as I could judge the weight of the decomposed air in the moisture acquired by the paper. . . . I wished, however, to have had a nicer balance for this purpose; the result was such as to afford a strong presumption that the air was reconverted into water, and therefore that the origin of it had been water." These results, together with those on the conversion of water into air, were communicated towards the end of March 1783 by Priestley to Watt, who began to theorise upon them, and then to put his thoughts together in the form of a letter to Priestley, dated April 26, 1783, and which he requested might be read to the Royal Society on the occasion of the presentation of Priestley's memoir. In this letter Watt says: "Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogisticated air. These two kinds of air unite with violence, they become red-hot, and upon cooling totally disappear. When the vessel is cooled, a quantity of water is found in it equal to the weight of the air employed. This water is then the only remaining product of the process, and *water, light, and heat* are all the products. *Are we not then authorised to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light, etc.?*"

This letter, although shown to several Fellows of the Society, was not publicly read at the time intended. Priestley, before its receipt, had detected the fallacy of his experiments on the seeming conversion of water into air, and as much of the letter was concerned with this

matter, Watt requested that it should be withdrawn. Watt, however, as he tells Black¹ in a letter dated June 23, 1783, had not given up his theory as to the nature of water, and on November 26, 1783, he restated his views more fully in a letter to De Luc. In the meantime Cavendish, having completed one section of his investigation, sent in a memoir to the Royal Society, which was read on January 15, 1784, in which he gives an account of his experiments, and announces his conclusion "that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston." Watt thereupon requested that his letter to De Luc should be published, and it was accordingly read to the Royal Society on April 29, 1784. Which of the two—Cavendish or Watt—is, under these circumstances, to be considered as "the true and first discoverer" of the compound nature of water is the question which has been hitherto the main subject of the Water Controversy.

Let us now consider the matter as it affects Lavoisier. In 1783 Lavoisier had publicly declared against the doctrine of phlogiston, or rather, as M. Dumas puts it, "against the crowd of entities of that name which had no quality in common except that of being intangible by every known method" (*Leçons sur la Philosophie Chimique*, p. 161). How completely Lavoisier had dissociated himself from the theory may be gleaned from his memoir of that year. "Chemists," he says, "have made a vague principle of phlogiston which is not

¹ Watt, *Correspondence*, p. 31.

strictly defined, and which in consequence accommodates itself to every explanation into which it is pressed. Sometimes this principle is heavy and sometimes it is not; sometimes it is free fire, and sometimes it is fire combined with the earthy element; sometimes it passes through the pores of vessels, and sometimes they are impenetrable to it: it explains at once causticity and non-causticity, transparency and opacity, colours and the absence of colours. It is a veritable Proteus which changes its form every moment."

But in reality Lavoisier had merely renounced one fetich for another. At the time that he penned these lines he was as much under the thraldom of *le principe oxygine* as the most devoted follower of Stahl was in the bondage of phlogiston. That the calcination of metals was but a slow combustion had been fully recognised. M. Berthelot tells us that as far back as the March of 1774 Lavoisier had written in his laboratory journal: "I am persuaded that the inflammation of inflammable air is nothing but a fixation of a portion of the atmospheric air, a decomposition of air. . . . In that case in every inflammation of air there ought to be an increase of weight," and he tried to ascertain this by burning hydrogen at the mouth of a vessel from which it was being disengaged. In the following year he asks, What remains when inflammable air is burnt completely? According to the theory by which he is now swayed it should be an acid, and he made many attempts to capture this acid. In 1777 he and Bucquet burnt six pints of the inflammable air from metals in a bottle containing lime-water, in the expectation that fixed air would be the result. And in 1781 he repeated the experiment with Gengembre, with the modification that the oxygen was caused to burn in an atmosphere of

hydrogen, but not a trace of any acid product could be detected. Of course there must have been considerable quantities of water formed in these experiments, but Lavoisier was preoccupied with the conviction that oxidation meant acidification, and the presence of the water was unnoticed, or, if noticed, was unheeded. Macquer, in 1776, had drawn attention to the formation of water during the combustion of hydrogen in air, but Lavoisier has stated that he was ignorant of that observation. What was it then that put him on the right track? We venture to think that M. Berthelot has himself supplied the answer. He says (p. 114): "Rumours of Cavendish's trials had spread throughout the scientific world during the spring of 1783. . . . Lavoisier, always on the alert as to the nature of the products of the combustion of hydrogen, was now in such position that the slightest hint would enable him to comprehend its true nature. He hastened to repeat his trials, as he had the right to do, never having ceased to occupy himself with a question which lay at the very heart of his doctrine."

"On the 24th of June 1783," continues M. Berthelot, "he repeated the combustion of hydrogen in oxygen, and he obtained a notable quantity of water without any other product, and he concluded from the conditions under which he had worked that the weight of the water formed could not be other than equal to that of the two gases which had formed it. The experiment was made in the presence of several men of science, among whom was Blagden, a member of the Royal Society of London, who on *this occasion* recalled the observations of Cavendish (*qui rappela à cette occasion les observations de Cavendish*)."

On the following day Lavoisier published his results.

The following is the official minute of the communication, translated from the register of the sittings of the Académie des Sciences :—

Meeting of Wednesday, 25th June 1783.

MM. Lavoisier and De Laplace announced that they had lately repeated the combustion of Combustible Air with Dephlogisticated Air ; they worked with about 60 pints of the airs, and the combustion was made in a closed vessel : the result was very pure water.

The cautious scribe who penned that minute did not commit himself too far. M. Berthelot, however, regards it as the first certain date of publication, established by authentic documents, in the history of the discovery of the composition of water ; “ a discovery,” he adds, “ which, on account of its importance, has excited the keenest discussion.”

You will search in vain through the laboratory journals, as given by M. Berthelot, for any indications either of experiments or reflections which would enable you to trace the course of thought by which Lavoisier was guided to the truth. There is absolutely nothing on the subject until in the eighth volume (25 mars 1783, au février 1784), and on p. 63 we come to the experiment of 24th June, and we read : “ In presence of Messieurs Blagden, of [name illegible], de Laplace, Vandermonde, de Fourcroy, Meusnier, and Legendre, we have combined in a bell-jar dephlogisticated air and inflammable air drawn from iron by means of sulphuric acid, etc. . . . The amount of water may be estimated at 3 drachms : the amount which should have been obtained was 1 ounce 1 drachm and 12 grains. Thus we must suppose that there was a loss of two-thirds of the amount of the air, or that there has been a loss of weight.”

And this is the experiment which, according to M. Berthelot, enabled Lavoisier to conclude that "the weight of the water formed could not be other than equal to that of the two gases which had formed it"! It is on this single experiment, hurriedly and imperfectly done, that Lavoisier's claim to the discovery of the compound nature of water is based. M. Berthelot objects to the assumption that it was hurriedly done. He says, on p. 114: "Lavoisier caused a new apparatus to be made, with a couple of tubes and two reservoirs for the gases; an arrangement which would require a certain amount of time to put together; this circumstance proves that it could not have been an improvised trial." To what extent it was improvised will be seen immediately.

Now although the laboratory journals do not in this case "inform us of Lavoisier's methods, and of the direction of his mind . . . the successive steps in the evolution of his private thought," we have other means of ascertaining how he arrived at his knowledge. The method was simplicity itself: *he was told of the fact, and his informant was none other than Cavendish's assistant, Blagden.*

Cavendish's memoir was published in 1784. Before it was struck off its author caused the following addition to be made: "During the last summer also a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water."

This addition, as I have had the opportunity of verifying by an inspection of the original MSS. in the archives of the Royal Society, was made in the handwriting of Cavendish's assistant and amanuensis, Blagden.

When Lavoisier's memoir appeared it was found to contain the following reference to this circumstance: "It was on the 24th of June that M. de Laplace and I made this experiment in presence of MM. le Roi, Vandermonde, and several other Academicians, and of Mr. Blagden, the present Secretary of the Royal Society of London. The latter informed us (*ce dernier nous apprit*) that Mr. Cavendish had already tried, in London, to burn inflammable air in closed vessels, and that he had obtained a very sensible quantity of water."

This reference was so partial, and its meaning so ambiguous, that Blagden addressed the following letter to Crell, to be published in his *Chemische Annalen* (Crell's *Annalen*, 1786, vol. i. p. 58).

It is so direct and conclusive that I offer no apology for giving it almost entire:¹—

I can certainly give you the best account of the little dispute about the first discoverer of the artificial generation of water, as I was the principal instrument through which the first news of the discovery that had been already made was communicated to Mr. Lavoisier. The following is a short statement of the history:—

In the spring of 1783 Mr. Cavendish communicated to me, and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us that out of them he must draw the conclusion that dephlogisticated air was nothing else than water deprived of its phlogiston; and, *vice versa*, that water was dephlogisticated air united with phlogiston. About the same time the news was brought to London that Mr. Watt, of Birmingham, had

¹ Mr. Muirhead's translation. *Vide* Watt, *Correspondence, Composition of Water*, p. 71.

been induced by some observations to form a similar opinion. Soon after this I went to Paris, and in the company of Mr. Lavoisier and of some other members of the Royal Academy of Sciences I gave some account of these new experiments and of the opinions founded upon them. They replied that they had already heard something of these experiments, and particularly that Dr. Priestley had repeated them. They did not doubt that in such manner a considerable quantity of water might be obtained, but they felt convinced that it did not come near to the weight of the two species of air employed, on which account it was not to be regarded as water formed or produced out of the two kinds of air, but was already contained in and united with the airs, and deposited in their combustion. This opinion was held by Mr. Lavoisier, as well as by the rest of the gentlemen who conferred on the subject; but, as the experiment itself appeared to them very remarkable in all points of view, they unanimously requested Mr. Lavoisier, who possessed all the necessary preparations, to repeat the experiment, on a somewhat larger scale, as early as possible. This desire he complied with on the 24th June 1783 (as he relates in the latest volume of the Paris memoirs). From Mr. Lavoisier's own account of his experiment, it sufficiently appears that at that period he had not yet formed the opinion that water was composed of dephlogisticated and inflammable airs, for he expected that a sort of acid would be produced by their union. In general, Mr. Lavoisier cannot be convicted of having advanced anything contrary to truth; but it can still less be denied that he concealed a part of the truth; for he should have acknowledged that I had, some days before, apprised him of Mr. Cavendish's experiments, instead of which the expression "*il nous apprit*" gives rise to the idea that I had not informed him earlier than that very day. In like manner Mr. Lavoisier has passed over a very remarkable circumstance—namely, that the experiment was made in consequence of what I had informed him of. He should likewise have stated in his publication not only that Mr. Cavendish had obtained "*une quantité d'eau très sensible*," but that the water was equal to the weight of the two airs added together. Moreover, he should have added that I had made him acquainted with Messrs. Cavendish and Watt's conclusions—namely, that water, and not an acid, or any other substance, arose from the combustion of the inflammable and dephlogisticated airs. But *those* conclusions

opened the way to M. Lavoisier's present theory, which perfectly agrees with that of Mr. Cavendish, only that Mr. Lavoisier accommodates it to his old theory, which banishes phlogiston. . . . The course of all this history will clearly convince you that Mr. Lavoisier (instead of being led to the discovery by following up the experiments which he and Mr. Bucquet had commenced in 1777) was induced to institute again such experiments, solely by the account he received from me, and of our English experiments ; and that he really discovered nothing but what had before been pointed out to him to have been previously made out and demonstrated in England.

To this letter, reflecting so gravely on his honour and integrity, Lavoisier made no reply. Nor did Laplace, Le Roi, Vandermonde, or any one of the Academicians concerned, vouchsafe any explanation. *De non apparentibus et de non existentibus eadem ratio.* No explanation appeared, because none was possible. M. Berthelot ignores this letter, which is the more remarkable, since reference is made to it in more than one of the publications which he tells us he has consulted in the preparation of his account of the Water Controversy. If he knew of it, he must regard it either as unworthy of an answer or as unanswerable.

It would be heaping Ossa on Pelion to adduce further evidence from letters of the time of what Lavoisier's contemporaries thought of his claims. *De mortuis nil nisi bonum.* I would much more willingly have dwelt upon the virtues of Lavoisier, and have let his faults lie gently on him ; but I have felt it incumbent on me on this occasion to make some public answer to M. Berthelot's book, and in no place could that answer be more fittingly given than in this town, which saw the dawn of that work out of which these grand discoveries arose. It may be that much of what I have had to say is as a twice-told tale to many of

you. I trust I need make no apology on that account. The honour of our ancestors is in our keeping, and we should be unworthy of our heritage and false to our trust if we were slow to resent or slack to repel any attempt to rob them of that glory which is their just right and our proud boast.

ADDENDUM

Translations of the foregoing Address appeared in several French periodicals devoted to popular science, accompanied by criticisms, for the most part hostile. The *Revue Scientifique* of 25th October 1890 also published the Address, to which, on the invitation of the editor, M. Charles Richet, M. Berthelot prefixed a letter, which may be translated as follows :—

I have no direct concern in the republication of Mr. Thorpe's address which you purpose making in the *Revue*. Personally, I have not any reason to complain of his courtesy, and I should have been silent so far as he is concerned, holding that one is not bound to enter into a controversy which is purely critical, where no new fact is alleged, and where the judgment of public opinion suffices to set things in their true place; however, I comply with your request to let your readers know what my opinion is.

To my mind, nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. All civilised nations are at one in proclaiming the glory of Newton, the greatest of astronomers, and yet the majority of English men of science, refusing to treat his rivals with equity, are not agreed to recognise Leibnitz's rights to the invention of the differential calculus: they are as prejudiced in this respect as was Newton himself. Something analogous occurs in regard to the discoveries which created modern chemistry a hundred years ago.

Unquestionably, Priestley and Cavendish are recognised by all as great discoverers. I have myself taken pains to describe Priestley's discovery of the principal gases in terms of admiration (*La Révolution Chimique*, p. 39), and especially that of oxygen, which

I unreservedly attribute to him (pp. 61, 62). I have also detailed, with the encomiums which they merit, the investigations of Cavendish, "one of the most powerful scientific minds of the last century," and particularly his fruitful research on (to use Blagden's phrase) the artificial generation of water. But the well-merited praise accorded to these English *savants* does not prevent some of their countrymen from persistently denying the right of Lavoisier to the discovery and co-ordination of those general ideas on which rests our actual conception of matter, more especially in relation to the composition of air and water. This, I venture to repeat, is an incident in the long-standing feud, continually being renewed in the history of science, between the sagacious discoverers of particular facts and the men of genius who frame general theories. The opinion of most Continental men of science seems, however, to be decided on this special point, as may be seen from the judgment given, not only by Dumas, but by Hoefer in his *History of Chemistry*, by H. Kopp in his careful account of the discovery of the composition of water, and by many others. I have merely concurred with them.

It was in this spirit that I sought to trace the history of the discoveries which constituted the doctrine of modern chemistry, by faithfully reproducing all its phases, whilst at the same time indicating the continuity of sequence in the facts and the paternity of the ideas. I did this with an impartiality which has brought upon me the reproach that I have been indifferent to the reputation of my countrymen—the very opposite to the accusations which are now directed against me.

As regards the composition of air, it is easy to separate facts from ideas. It is certain that the discovery of oxygen is due to Priestley. But, said Lavoisier, "If I am reproached for having borrowed my proofs from the works of this celebrated philosopher, at least none will contest my right to the conclusions, which are often diametrically opposed to his."

Priestley, obstinately adhering to the theory of phlogiston, regarded his new gas as consisting of the very substance of air deprived simply of its phlogiston; whilst nitrogen, according to him, was formed also of this same substance combined with a complementary portion of phlogiston. He remained faithful to this doctrine, which obscured the true nature of the greater number of chemical phenomena, until the moment when, like Lavoisier,

persecuted by his countrymen, who now proclaim his fame, driven from home, his laboratory burnt by a mob, and threatened with death, he fled to America, where he died in sadness and in solitude. Even more unfortunate was Lavoisier !

But whatever may have been the personal fate of these two great men, if it is true that Priestley discovered oxygen, it is not the less certain that the true theory of the nature of air is due to Lavoisier.

The history of the composition of water is more complicated. In reality the discovery of the facts belongs neither wholly to Cavendish—who undoubtedly played a most important part, inasmuch as he gave the impetus towards the definitive solution—nor to Lavoisier, who first established a knowledge of the facts by his public experiments and his published writings—nor even to the two combined. They had predecessors, and at the moment even when the light came, Monge played an essential part in the rigorous demonstration of which Mr. Thorpe apparently has no suspicion. Thus each man's share in this history cannot be settled by a word : we require to follow exactly the gradual progress of experiment and publication. But here, again, if Lavoisier is not the principal discoverer of the facts, it is he who has the incontestable merit of having furnished the exact interpretation of the phenomena, freed from the mists of phlogiston, to which Cavendish seems to have remained faithful to the day of his death.

I have elsewhere laid bare all these facts, and I have no intention of reproducing here the details of a controversy exhausted even in Lavoisier's time, and in which Mr. Thorpe does no more than reproduce the unjustifiable imputations of Blagden, who, impelled by passion, went so far as to interpolate and falsify, with his own hand, the manuscript memoirs of Cavendish, in order to gain arguments in support of his accusations.

Moreover, nothing more decisively establishes the part played by Lavoisier, and his right to the institution of our modern theories, than the letter of a contemporary English *savant*, Black, as celebrated for his discoveries in physics as in chemistry, and who might have put forward claims on his own account. In 1791 he wrote to Lavoisier, in a letter equally honourable to both :—"The numerous experiments which you have made on a large scale, and which you have so well devised, have been pursued with so much care and with such scrupulous attention to details, that nothing can

be more satisfactory than the proofs you have obtained. The system which you have based on the facts is so intimately connected with them, is so simple and so intelligible, that it must become more and more generally approved and adopted by a great number of chemists who have long been accustomed to the old system. . . . Having for thirty years believed and taught the doctrine of phlogiston as it was understood before the discovery of your system, I, for a long time, felt inimical to the new system, which represented as absurd that which I had hitherto regarded as sound doctrine; but this enmity, which springs only from the force of habit, has gradually diminished, subdued by the clearness of your proofs and the soundness of your plan."

We can but hope to see the day when the scientific men of England will conform to the opinion of one of the most illustrious of their countrymen.

M. BERTHELOT, of the Institute.

Certain passages in M. Berthelot's letter, more especially those reflecting on the character of Blagden, seemed to me to require some notice. I therefore addressed the following remarks to *Nature* of 6th November 1890:—

I quite agree with M. Berthelot that nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. It was for that reason that I felt compelled, in the Leeds address, to protest against the spirit and bias of the accounts of the discovery of the facts relating to oxygen and the composition of water given in *La Révolution Chimique*. Although M. Berthelot's letter somewhat confuses the issues, there is, in reality, but small difference between us. What I ventured to criticise was the general tone and tendency of M. Berthelot's argument, which seems to palliate, and even to justify, Lavoisier's pretensions to a discovery in which he has no right even to be considered as a participator. M. Berthelot now tells us in his letter that he attributes

the discovery of oxygen unreservedly to Priestley. So far so good. It is something gained to have thus secured such an unqualified statement from one who occupies the position of authority in the world of chemistry in France that is enjoyed by the present Perpetual Secretary of the Academy. We may well hope, therefore, that this particular question has been finally set at rest.

M. Berthelot need not ask British men of science to conform to the opinion of Black. They already do so. That to Lavoisier, and to Lavoisier alone, belongs the merit of having effected the overthrow of the theory of phlogiston, and of having to that extent laid the foundation of modern chemistry, is not questioned on this side of the Channel. So far as I know, it has only been among Lavoisier's own countrymen that any doubt on this point has been raised. We all remember the passionate scorn with which Lavoisier repudiated and protested against the attempts of his compatriots to rob him of his rights: "*Cette théorie n'est donc pas comme je l'entends dire—la théorie des chimistes français; elle est la mienne, et c'est une propriété que je réclame auprès de mes contemporains et de la postérité.*" It is true, as M. Berthelot implies, that Black has claims. Lavoisier himself admits as much. It would be easy, if it were not beside the points at issue, to match the letter which M. Berthelot quotes by others from Lavoisier in which he ascribes to Black the germs of his doctrine. M. Berthelot, I repeat, confuses the issues. This particular point was never raised by me in the address. What I said was: "Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen, and the determination of the compound nature of water. M. Berthelot's contention

is, that not only did Lavoisier effect the overthrow, but he also discovered the facts." I, in common, I venture to assert, with every British chemist, admit unreservedly that Lavoisier effected the overthrow, but we deny that he discovered the facts. It is altogether beside the question for M. Berthelot now to say in effect: "Have I not praised your men of science, and thereby drawn down upon myself the wrath of my countrymen? And yet you are not satisfied!" We are sorry for M. Berthelot; he is in the position of the man with many friends, and his friends for the moment are a little angry. He has either not the courage of his convictions, or he has halted between two opinions—with the usual consequences.

With respect to the discovery of the compound nature of water, M. Berthelot now takes up a different position from that which he occupies in *La Révolution Chimique*. His contention there was that by every legitimate canon the experiment of 24th June 1783 gives to Lavoisier the priority of discovery. He now admits that Cavendish played "un rôle capital—car il donna le branle aux esprits vers la solution définitive." But how was this possible when Cavendish's memoir was not published until January 1784? There is really only one answer—it was given simply by the intervention of Blagden. I repeat that Blagden told Lavoisier of Cavendish's researches and of his conclusions, and that it was in the light of that knowledge that the experiment of 24th June 1783 was made. There can be no question of this. Blagden's testimony, as given in the letter to Crell, is as direct and decisive as it is damning. It was never contradicted by Lavoisier, nor by Laplace, Vandermonde, Fourcroy, Meusnier, or Legendre, who were present on the

occasion when Lavoisier himself admits that he received the information. M. Berthelot does not contradict it, but, instead, he asperses the moral character of Blagden. This method of treating a witness whose evidence cannot be rebutted is apt, when unsuccessful, to recoil on him who attempts it. It is perfectly true that Blagden interpolated the famous passage in Cavendish's memoir :—

During the last summer, also, a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them. . . . But at that time, so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water.

This passage, however, was inserted with Cavendish's knowledge and consent, and by his assistant and amanuensis, who happened to be the very man who had a personal knowledge of the facts. Assuming the statement to be true, where is the immorality of the proceeding?

Everything that we can learn authoritatively concerning Blagden goes to show that he was an upright and honourable man. Sir Joseph Banks has testified to his abilities and integrity. Dr. Johnson spoke of his copiousness and precision of communication, with the characteristic addition : "Blagden, sir, is a delightful fellow." Laplace, Cuvier, Berthollet, and Benjamin Delessert were among his friends.¹ He was rich, and

¹ Many of the letters of Berthollet to Blagden are still in existence. In one of these, dated "19 Mars, 1785," he writes from Paris :—"L'on s'est beaucoup occupé ici ces derniers tems de la belle découverte de Mr. Cavendish, sur la composition de l'eau : Mr. Lavoisier a tâché de porter sur cet objet toute l'exactitude dont il est susceptible. . . . Mr. Lavoisier veut répéter l'expérience en faisant brûler l'air dephlogistiqué dans le gas inflammable, et il y a apparence qu'alors on n'aura point d'acide nitreux, selon les belles observations de Mr. Cavendish." Is this language consistent with the belief that Berthollet, who must have known the facts, regarded Lavoisier as the real discoverer of the compound nature of water?

was understood to have speculated to profit in the French funds. For thirteen years he was a Secretary of the Royal Society, and in 1792 he was knighted for his services to science. Every year he spent a considerable time on the Continent, and was frequently in Paris. The gossip of the period states that he aspired to the hand of Madame Lavoisier, who preferred Count Rumford. He died in Berthollet's house at Arcueil, on 26th March 1820. In an obituary notice in the *Moniteur* of 22nd September 1820, M. Jomard testifies to his benevolence and generosity, and states that "none of his countrymen have done more justice to the labours and discoveries of the French, or have contributed more than he to the happy relations which have subsisted for six years (1814-20) between the *savans* of the two countries." By his will he provided liberally for his scientific friends: Berthollet, the daughter of Madame Cuvier, and the daughter of Count Rumford each received £1000; and Laplace £100, "to purchase a ring." M. Berthelot asperses the character, not only of Blagden, but also of his countrymen, by his insinuations. Would he have us believe that Berthollet, Cuvier, and Laplace would extend their friendship to, and receive pecuniary benefits from, one whom they believed had foully stabbed their compatriot in the back? It is surely incumbent on M. Berthelot, on every ground, either to substantiate his implications or to withdraw them.

M. Berthelot makes the gratuitous assumption that I am ignorant of the work of Monge. Whether I am or not is altogether beside the mark. There is, indeed, no question of Monge. Monge distinctly disclaims priority to Cavendish, nor did he attempt to establish a right to be considered an independent discoverer of

the true nature of water. In his memoir, "*Sur le Résultat de l'Inflammation du Gas inflammable et de l'Air dephlogistiqué dans les Vaisseaux Clos*," he tells us that the experiments recorded in it were made in June and July 1783, and repeated in October of the same year. "I did not then know," he adds, "that Mr. Cavendish had made them several months before in England, though on a smaller scale; nor that MM. Lavoisier and Laplace had made them about the same time at Paris in an apparatus which did not admit of as much precision as the one which I employed." I fail to see what M. Berthelot gains by his reference to Monge.

M. Berthelot reproaches Priestley and Cavendish for their adherence to phlogistonism. I say it with all respect—but is it seemly for M. Berthelot, of all men, to cast this stone? Is not he himself an exemplification of that conservatism which he deplores? A generation ago the doctrine of Avogadro became the corner-stone of that edifice of which M. Berthelot asserts that Lavoisier laid the foundations. Indeed, the introduction of that doctrine effected a revolution hardly less momentous than that of which Lavoisier was the leader. But what has been M. Berthelot's consistent attitude towards this teaching? We can illustrate it by a single example. He is the sole teacher in Europe of any position who continues to symbolise the constitution of that very substance of which he claims that Lavoisier discovered the composition by a formula which is as obsolete as any conception of phlogistonism.

VIII

MICHAEL FARADAY

A REVIEW OF DR. BENGE JONES'S "LIFE AND LETTERS OF FARADAY."
—*MANCHESTER GUARDIAN*, 1870

MICHAEL FARADAY, one of the greatest experimental philosophers of this or indeed of any other century, was born at Newington, in Surrey, on the 22nd September 1791. Shortly after the birth of Michael, their third child, his parents settled permanently in London; but through the continued ill-health of the father, who was a blacksmith by trade, the family were always in straitened circumstances. During the distress of 1801 they received public relief, and to the little Michael one loaf of bread was given each week, and it had to serve him for that length of time. Of his mother Faraday always spoke in the most affectionate terms, and to her care and solicitude may be attributed the great influence which his home had upon his character. Although unable to enter into his occupations, she was exceedingly proud of her son; so much so that Faraday asked his wife not to talk to his mother so much about him or his honours, saying she was quite proud enough of him, and it would not be good for her. Poor as the parents were, they managed to afford their children some little school learning, and Michael obtained the rudiments of reading, writing, and arithmetic at a common day-

school. When twelve years old the young Faraday went on trial for a year as an errand-boy to Mr. George Riebau, a bookseller and bookbinder in Blandford Street, near Manchester Square. He tells us "that it was his duty, when he first went, to carry round the papers that were lent out by his master. Often on a Sunday morning he got up very early and took them round, and then he had to call for them again; and frequently, when he was told the paper was not done with, 'You must call again,' he would beg to be allowed to have it, for his next place might be a mile off, and then he would have to return over the ground again, losing much time and being very unhappy if he was unable to get home to make himself neat, and to go with his parents to their place of worship." The Faradays were members of a Sandemanian congregation, and to this sect Faraday adhered throughout his life, and for many years was an elder of their chapel.

We are told that in after-life the remembrance of his earliest occupation was often brought to the mind of Faraday. One of his nieces said that he rarely saw a newspaper-boy without making some kind remark about him. "I always feel a tenderness for those boys," he said on one occasion, "because I once carried newspapers myself."

The year of probation having expired, Faraday was apprenticed to his master, who, as it is written in the indentures, required no premium in consideration of his previous faithful service. The young Faraday was by no means precocious in disposition, but his great originality of mind quickly showed itself. Few books passed under his hands without his obtaining some knowledge of their contents. Watts *On the Mind*, he said, first made him think, and Mrs. Marcet's *Conversa-*

tions on Chemistry, and the article "Electricity," in an encyclopædia he was employed to bind, first turned his attention to science. His amiability, and the intelligent nature of his conversation, soon attracted the attention of his master's customers; and one of them, a Mr. Dance, afforded him the opportunity of hearing Sir Humphry Davy lecture at the Royal Institution. Davy was then near the zenith of his fame; his brilliant discovery of the compound nature of the alkalis and alkaline earths created an epoch in the history of chemistry, and stamped the discoverer as one of the greatest workers of his time. Probably no English philosopher ever enjoyed a higher degree of popularity than did Davy about this time. Science was just then in fashion, and chemistry all the rage. Albemarle Street was often blocked with the carriages of the fine ladies and gentlemen who thronged to hear the great chemist expound the new theories which his own discoveries had helped to inaugurate. Some may think that there was much in the career of Davy to have excited the young Faraday's aspirations; in their origin at least the baronet and the bookbinder had something in common, and by the same steps by which the one had risen the other might mount. But it is more than probable that then, and for a long time subsequently, the desire to emulate the titled lecturer never once crossed the mind of his humble auditor, perched over the clock in the gallery. Davy's example may have stimulated his hopes, but it did not create them. The bias to his inclinations had already been given. Faraday's occupation as a bookbinder was going, if not gone. His longing to escape from trade continually breaks out in his correspondence. "The desire," he says, "to be engaged in scientific occupation,

even though of the lowest kind, induced me to write, in my ignorance of the world and simplicity of my mind, to Sir Joseph Banks, then President of the Royal Society. Naturally enough 'no answer' was the reply left with the porter." His thoughts at this time, when he was "giving up trade and taking to science," are well seen in his letters to his friend Abbott. Mr. Abbott was a confidential clerk in the City, whose acquaintance Faraday had made during a course of lectures on natural philosophy given by Mr. Tatum. Abbott was apparently well educated, and the characteristic deference paid by Faraday to his friend's superior school-learning may repeatedly be seen in the course of their long correspondence. "These letters to Abbott," says Dr. Bence Jones, "possess an interest almost beyond any other letters which Faraday afterwards wrote. It is difficult to believe that they were written by one who had been a newspaper-boy, and who was still a bookbinder's apprentice, not yet twenty-one years of age, and whose only education had been the rudiments of reading, writing, and arithmetic. Had they been written by a highly educated gentleman, they would have been remarkable for the energy, correctness, and fluency of their style, and for the courtesy, kindness, candour, deference, and even humility of the thoughts they contain." This is high praise, but it is fully merited. Some of the letters are indeed models of epistolary style.

Faraday was not content with simply listening to Davy, but made careful notes of the lectures, afterwards writing them out in a fuller form, and interspersing them with such drawings as he could make. His own words to Dr. Paris, the biographer of Davy, written seventeen years after, will best show what he then did

with these notes: "My desire to escape from trade, which I thought vicious and selfish, and to enter into the service of science, which I imagined made its pursuers amiable and liberal, induced me at last to take the bold and simple step of writing to Sir Humphry Davy, expressing my wishes, and a hope that if an opportunity came in his way he would favour my views; at the same time I sent the notes I had taken of his lectures." Davy, at their first meeting, wished to dissuade the young aspirant from the course which he contemplated taking, telling him that science was a harsh mistress, and in a pecuniary point of view but poorly rewarded those who devoted themselves to her service. He smiled at Faraday's notion of the superior moral feelings possessed by scientific men, and said that a few years' experience would set him right on this matter;—which it most certainly did, as we shall hereafter show. Some time after this interview Faraday was startled late at night by a loud knock at the street door, and was still more astonished to see a footman alight from a carriage and leave a note for him. The note was from Sir Humphry to inform him that the position of laboratory-assistant at the Royal Institution was then vacant, and if he were still minded to leave his present occupation, the place was at his disposal. A few days after Faraday commenced his duties at Albemarle Street at a salary of 25s. a week, with two rooms at the top of the house.

Such was the origin of his connection with the Royal Institution—a connection which lasted nearly to the end of his long life, and which raised the Institution to the eminence it at present occupies. The very nature of his first duties there shows that he must have already acquired no inconsiderable amount of manipulative skill

from the experiments which he made in the course of his self-tuition in Blandford Street. Davy was at that time engaged on the study of the so-called nitrogen trichloride, perhaps the most explosive compound known to chemists. That he should allow Faraday to assist him in a research where a single blunder might be attended with the most serious consequences, indicates that Davy, himself no mean experimenter, fully recognised the other's ability. Throughout the course of the investigation Faraday was exposed to constant peril, and it was only through continual care and precaution that no very sad results attended the numerous explosions of this singularly unstable compound.

In the autumn of this year (1813) Sir Humphry Davy proposed to go abroad, and offered to take Faraday with him as amanuensis, promising that he should resume his situation at the Institution upon his return. During his travels on the Continent with Davy, which lasted about a year and a half, Faraday kept a journal, in which he noted down his impressions of the journey. This journal, which occupies a considerable portion of the first volume of Dr. Bence Jqnes's work, is as remarkable for the vividness and accuracy of the descriptions of what he saw, as for the entire absence of any particulars relating to those with whom he travelled. His cautious reticence on this latter point is scarcely less apparent in his letters; but the little that does escape him shows that his journey was not one of unmixed pleasure. His relation to Sir Humphry Davy was not very clearly defined; to his duties as an amanuensis he was not unfrequently obliged to add those of a valet, and this service was especially annoying to him, the more so that it was entirely unexpected. Perhaps no one ever possessed more real humility than did Faraday, and the

very genuineness of his humility is shown by its never degenerating into servility. In one of his letters to Abbott he writes : " I fancy I have cause to grumble, and yet I can scarcely tell why. If I approve of the system of etiquette and valuation formed by the world, I can make a thousand complaints ; but, perhaps, if I acted influenced by the pure and unsullied dictates of common sense, I should have nothing to complain of, and therefore all I can do is to give you the circumstances." He then relates how it came to pass that he was obliged to undertake duties so irksome to him. But he concludes by saying that it was, perhaps, the name more than the thing which hurts. " I should have but little to complain of were I travelling with Sir Humphry alone, or were Lady Davy like him ; but her temper makes it oftentimes go wrong with me, with herself, and with Sir Humphry." In another letter he writes : " I am quite ashamed of dwelling so often on my own affairs, but as I know you wish it, I shall briefly inform you of my situation." . . . He then goes on to say that Sir Humphry was unable to meet with a valet to his satisfaction. " This, of course, throws things into my duty which it was not my agreement, and is not my wish, to perform, but which are, if I remain with Sir Humphry, unavoidable. These, it is true, are very few ; for, having been accustomed in early years to do for himself, he continues to do so at present, and he leaves very little for a valet to perform ; and as he knows that it is not pleasing to me, and that I do not consider myself as obliged to do them, he is always as careful as possible to keep those things from me which he knows would be disagreeable. But Lady Davy is of another humour. She likes to show her authority, and at first I found her extremely earnest in mortifying me.

This occasioned quarrels between us, at each of which I gained ground and she lost it; for the frequency made me care nothing about them, and weakened her authority, and after each she behaved in a milder manner."

But his ardent desire for improvement and the opportunity he possessed of adding to his knowledge of chemistry and of the other sciences continually induced him to proceed. At Geneva Faraday met Volta, but the only record of his interview with the great electrician is contained in the following lines:—"Friday 17th. Saw M. Volta, who came to Sir Humphry Davy, a hale, elderly man, bearing the red ribbon, and very free in conversation." If the young and diffident amanuensis could have foreseen the glorious destiny that awaited him, what emotions this meeting must have raised. At Geneva Faraday also made the acquaintance of De La Rive, who, "undazzled by the brilliancy of Davy's reputation, was able to see the true worth of his assistant." This led him on one occasion to place Faraday even then on a par with Davy. He invited them both to dinner. Davy, it is said, declined to dine with one who, in some things, acted as his servant. De La Rive expressed his feelings only by saying that it would then be necessary for him to give two dinners instead of one. In 1858 Faraday wrote to Mr. A. De La Rive: "I have some such thoughts (of gratitude) even as regards your own father, who was, I may say, the first who personally at Geneva, and afterwards by correspondence, encouraged, and by that sustained me."

Having thus travelled through France, Italy, Switzerland, and the Tyrol, Faraday returned to England in the spring of 1815, and immediately resumed his duty at the Royal Institution. Davy could no longer withhold

the acknowledgment of the energy and worth of his assistant. On the other hand, Faraday had now full knowledge of "his master's genius and power. He had compared him with the French philosophers whilst helping him in his discovery of iodine; and he was just about to see him engage in those researches on fire-damp and flame which ended in the glorious invention of the Davy Lamp, and gave to Davy a popular reputation even beyond that which he had gained in science by the greatest of all his discoveries—potassium." Dr. Jones is slightly in error here; iodine was discovered in 1812 by Courtois, a saltpetre manufacturer in Paris; Sir Humphry Davy, however, first conclusively demonstrated its elementary nature.

In the following year Faraday commenced lecturing. He delivered his first lecture on January 17, 1816, at the City Philosophical Society, on "the general properties of matter." In the very full notes which Dr. Bence Jones has preserved to us of this and of the subsequent lectures delivered during the year at the same Society, we think we can trace the germs of that great success which attended Faraday's career as a lecturer. These notes are remarkable for their originality of thought and fulness of illustration. In his accurate digests of contemporary knowledge we have evidence of the diligence with which Faraday set about educating himself for his new vocation. Lecturing became with him an art, to be carefully and systematically studied. His letters to Abbott, and his commonplace book, show that he had previously framed pretty accurate ideas respecting the province and functions of the lecturer whilst attending Mr. Brande and Mr. Powell in their lectures at the Royal Institution. He now took private lessons in elocution, and prevailed

on his teacher to attend his lectures in order that the errors of his address and delivery might be brought home to him. Eleven years after the date of this early attempt, Faraday delivered his first lecture at the Royal Institution. As he steps upon the place where, fifteen years before, Davy had stood, we wonder if his thoughts revert to the memory of the young bookbinder's apprentice, seated in the gallery over the clock! "For thirty-eight years," says his biographer, "his lectures were the life of the Royal Institution. His singular power of making himself one of his audience was felt in his juvenile lectures, in his theatre courses, and in his Friday evening addresses. In his juvenile lectures, his simple words and his beautiful experiments, his quickness and his clearness, kept the attention and fixed his instruction in the mind even of the youngest of his hearers, whilst the most practised teacher would find old experiments shown in a new form, which the genius of Faraday only could have invented, and which his handicraft enabled him to carry out. In his theatre lectures his matter was always over-abundant, his experiments were always successful, his knowledge was always at the furthest limits to which it had at the time been extended by himself or by others, and yet his consideration for those who knew but little never failed. But it was in his Friday evening discourses that his great power as a lecturer came out. His manner was so natural that the thought of any art in his lecturing never occurred to any one. Rapidly, and yet clearly, he made the object of his lecture known. Those who had but little knowledge could see his starting-point, and they thought they saw where he was going. Those who knew most followed him beyond the bounds of their own knowledge, forgetting almost the lecturer, who

seemed to forget himself, in his words and his experiments, and who appeared to be trying only to enable them to judge what his latest discoveries were worth; and when he brought the discoveries of others before his hearers, one object, and one alone, seemed to determine all he said and did, and that was, 'without commendation and without censure, to do the utmost that could be done for the discoverer.'"

In 1821, when twenty-nine years of age, Faraday married Miss Sarah Barnard, the daughter of an elder of the Sandemanian Chapel. Many years after he wrote in the notes of his own life: "On June 12, 1821, he married—an event which more than any other contributed to his earthly happiness and healthful state of mind. The union has continued for twenty-eight years, and has nowise changed, except in the depth and strength of its character."

About this time Faraday received his first scientific honour; it came from the Cambridge Philosophical Society. Perhaps no scientific man ever obtained during his lifetime such an extended recognition of his services as did Faraday; in all he received no less than ninety-five honorary titles and marks of merit from the various learned societies scattered throughout Europe and America. In 1838 he wrote: "One title, that of F.R.S., was sought and paid for; all the rest are spontaneous offerings of kindness and goodwill." Writing, in 1854, to Lord Wrottesley, then Chairman of a Parliamentary Committee of the British Association, he says: "Through the kindness of all, from my Sovereign downwards, I have that which supplies all my need; and in respect of honours I have, as a scientific man, received from foreign countries and sovereigns those which, belonging to very limited and

select classes, surpass, in my opinion, anything that it is in the power of my own to bestow. I cannot say that I have not valued such distinctions; on the contrary, I esteem them very highly, but I do not think that I have ever worked for or sought after them."

In 1829 he was made a member of the Scientific Advising Committee of the Admiralty, and from about this time dates his connection with the Government, which lasted nearly to the end of his life. To Lord Auckland he wrote: "I have always, as a good subject, held myself ready to assist the Government if in my power—not for pay, for except in one instance (and then only for the sake of the person joined with me), I refused to take it." The exception to which he here refers was in the case of the Haswell Colliery explosion, when he was sent down by the Government, together with Sir (then Mr.) Charles Lyell, to attend the inquest. Faraday, however, squared the account with his conscience by subscribing most liberally to the fund raised for the widows and children of those who had perished in the catastrophe.

In 1836 he was appointed Scientific Adviser to the Trinity House; and his work, extending over a period of thirty years, was of the most miscellaneous character, including "the ventilation of lighthouses; the arrangements of their lightning conductors; the analysing and supervising of their drinking waters; the examination of their optical apparatus; the determination of the worth of the different propositions made to the Trinity House regarding the lights, extending from the practical use of the magneto-electric light down to the samples of cottons, oils, and paints that were used." When he was appointed he spoke to the deputy-master "of his indifference to his proposition as a matter of interest,

though not as a matter of kindness." The value of this appointment was in the words of Dr. Jones, "an unlimited amount of kindness and £200 a year."

In 1816 Faraday made his first contribution to science. It consisted of an analysis of a specimen of native caustic lime. Sir Humphry Davy had given him the analysis to make as a first attempt in chemistry "at a time," as he tells us, "when his fear was greater than his confidence, and both far greater than his knowledge; at a time, also, when he had no thought of ever writing an original paper on science." His activity of mind at this time was marvellous. From the date of his first paper until 1820 he contributed no less than thirty-seven notices and papers to the *Quarterly Journal of Science*.

But the work which made Faraday the wonder of the scientific world was yet to be done, and the records of the next eleven years show how carefully and how patiently he educated himself for his great task. In 1823 he published his first paper on the condensation of the gases. The first gas which he liquefied was chlorine. Acting upon a suggestion made by Davy, Faraday, who was then working upon the hydrate of chlorine, sealed up some of the crystals of this substance in a bent tube, and subjected them to heat. The result was the formation in the bent portion of the tube of a quantity of a yellow oily liquid which, as Faraday "puzzled out" for himself, could only be condensed chlorine.

This discovery immediately attracted universal attention; but the merit of it was claimed by another. The claimant was no other than Davy. A feeling of jealousy had gradually been growing up in the mind of the Honorary Professor of Chemistry at the Royal

Institution towards his assistant, and this discovery exposed it to the world. Charges of plagiarism were freely whispered about, but it was only after the lapse of many years, when the question was again revived by the late Dr. Davy in the Life of his illustrious brother, that Faraday was prevailed upon to set the matter in its proper light. Faraday was always very much averse to scientific controversy, and it was difficult to move him to take up his pen in his own defence. Many years afterwards he attempted to mediate between Matteucci and Du Bois Raymond, who were at variance. Writing to the former, he says: "Who has not to put up in his day with insinuations and misrepresentation in the accounts of his proceedings given by others, bearing for the time the present injustice, which is often unintentional and often originates in hasty temper, and committing his fame and character to the judgment of the men of his own and future time. I see that that moves you which would move me most—namely, the imputation of a want of good faith; and I cordially sympathise with any one who is so charged unjustly. Such cases have 'seemed' to me almost the only ones for which it is worth while entering into controversy. I have felt myself not unfrequently misunderstood, often misrepresented, sometimes passed by, as in the cases of specific inductive capacity, magneto-electric currents, definite electrolytic action, etc.; but it is only in the cases where moral turpitude has been implied that I reply. . . . These polemics of the scientific world are very unfortunate things; they form the great stain to which the beautiful edifice of scientific truth is subject. Are they inevitable? They surely cannot belong to science itself, but to something in our fallen nature." Faraday's defence of himself in the affair of

the condensation of the gases is very explicit. It was written to his friend, Richard Phillips, and was published in the *Philosophical Magazine* for 1836. He concludes it as follows :—

I have never remarked upon or denied Sir H. Davy's share of the condensation of chlorine or the other gases ; on the contrary, I think that I long ago did him full justice in the papers themselves. How could it be otherwise ? He saw and revised the manuscripts ; through his hands they went to the Royal Society, of which he was president at the time ; and he saw and revised the printer's proofs. Although he did not tell me of his expectations when he suggested heating the crystals in a closed tube, yet I have no doubt that he had them ; and, though perhaps I regretted losing my subject, I was too much indebted to him for much previous kindness to think of saying that that was mine which he said was his. But observe (for my sake) that Sir H. Davy nowhere states that he told me what he expected, or contradicts the passages in the first paper of mine, which describe my course of thought, and in which I claim the development of the actual results. All this activity in the condensing of gases was simultaneous with the electro-magnetic affair, and I had learned to be cautious upon points of right and priority. When, therefore, I discovered in the course of the same year that neither I nor Sir H. Davy had the merit of first condensing the gases, and especially chlorine, I hastened to perform what I thought right, and had great pleasure in spontaneously doing justice and honour to those who deserved it. Monge and Clouet had condensed sulphurous acid probably before the year 1800 ; Northmore condensed chlorine in the years 1805 and 1806 (*Nicholson's Journal*). I therefore published on January 1 in the following year, 1824, a historical statement of the liquefaction of gases (*Quarterly Journal of Science*). . . . The value of this statement of mine has since been fully proved, for, upon Mr. Northmore's complaint, ten years after, with some degree of reason, that great injustice had been done to him in the affair of the condensation of gases, and his censure on the "conduct of Sir H. Davy, Mr. Faraday, and several other philosophers for withholding the name of the first discoverer," I was able, by referring to the statement, to convince him and his friends that if my papers had done him

wrong, I at least had endeavoured also to do him right (*Philosophical Magazine*, 1834). Believing that I have now said enough to preserve my own "honest fame" from any injury it might have risked from the mistakes of Dr. Davy, I willingly bring this letter to a close, and trust I shall never again have to address you on the subject.

Davy's jealousy culminated in active opposition to the election of Faraday as a Fellow of the Royal Society. His certificate as a candidate was drawn up by his friend Phillips, and the first signatures were those of Wollaston, Children, Babington, and Sir W. Herschel. Many years after Faraday gave the following account of what had passed between him and Davy relative to the matter of his election: "Sir H. Davy told me I must take down my certificate. I replied that I had not put it up; that I could not take it down, as it was put up by my proposers. He then said I must get my proposers to take it down. I answered that I knew they would not do so. Then he said: I, as president, will take it down. I replied that I was sure Sir H. Davy would do what he thought was for the good of the Royal Society." One of Faraday's proposers bears witness that "Sir H. Davy had walked for an hour round the courtyard of Somerset House arguing that Faraday ought not to be elected." Davy's prediction to the aspiring young bookbinder that his notions of the superior moral feelings of philosophic men would be set right by a few years' experience was signally verified. But on the ballot being taken, Faraday was almost unanimously elected; there was only one black ball.

About this time Faraday discovered benzene, or, as he then termed it, bicarburet of hydrogen, among the products of the condensed oil-gas manufactured by

the Portable Gas Company. Faraday may thus be said to have laid the foundation of that immense industry which recent discoveries in chemistry have so rapidly developed—the manufacture of the so-called aniline dyes. Enormous quantities of benzene are now employed in the production of these beautiful colours.

In 1831 the great work of his life commenced by the publication in that year of the first series of his immortal *Experimental Researches in Electricity*. Our limited space entirely prevents any attempt to do justice to the nature and extent of his researches on magneto-electricity, voltaic induction, definite electro-chemical decomposition; nor, we regret to add, can we refer our readers to the book before us for a more detailed account. In our opinion Dr. Bence Jones fails to present any clear conception of the nature of Faraday's discoveries. Dr. Jones has in one sense performed his duty as a biographer most conscientiously, perhaps rather too conscientiously; like the famous Cid Hamet, he is the most punctual and diligent searcher after the minutest circumstances, "even to the very atoms of his true history," and the result is that too much is left to the judgment and discrimination of his reader in the matter of Faraday's discoveries. Thus we see on the same page, with no attempt at distinction, an account of a paper on the general magnetic relations and characters of the metals, together with one on such a comparatively unimportant subject as a supposed new sulphate and oxide of mercury. Happily, however, Faraday does not want an interpreter. We can refer our readers to no clearer exponent of his labours than Professor Tyndall, whose charming book, *Faraday as a Discoverer*, is eminently worthy the attention

of those who desire to know more of the great philosopher's work.

We have already spoken of Faraday as a good subject, and we should like to dwell for a moment on the manner in which he fulfilled his duties as a good citizen. He took the warmest interest in all the great questions of the day. His desire for sanitary reform was the occasion of his letter to the *Times* on the state of the river Thames. Who is not familiar with Leech's cartoon of Faraday giving his card to Father Thames, with the hope that the "Dirty fellow will consult the learned professor"? Within recent years the question of the position of natural science in the various *curricula* of our schools and universities has attracted considerable attention. Faraday's opinion on this subject is especially valuable. "I do think," he says, "that the study of natural science is so glorious a school for the mind that, with the laws impressed on all created things by the Creator, and the wonderful unity and stability of matter and the forces of matter, there cannot be a better school for the education of the mind." When examined before the Public School Commissioners, he said: "That the natural knowledge which had been given to the world in such abundance during the last fifty years, I may say, should remain untouched, and that no sufficient attempt should be made to convey it to the young mind, growing up and obtaining its first views of these things, is to me a matter so strange that I find it difficult to understand; though I think I see the opposition breaking away, it is yet a very hard one to be overcome. That it ought to be overcome I have not the least doubt in the world."

In 1835 Faraday was told that Sir Robert Peel, then Prime Minister, contemplated offering him a pension of

£300 a year. His first impulse was to refuse it, on the ground that he could not accept a pension whilst he was able to work for his living. He was induced, however, by his relatives to reconsider his determination, and waited upon Lord Melbourne, who was then in office, to learn his intentions in the matter. During their conversation his lordship expressed himself, as he himself admits, in rather "too blunt and inconsiderate a manner." According to Dr. Jones it is probable that he designated the system of giving pensions to literary and scientific men as a piece of humbug. However, on the same evening, Faraday left his card with the following note at Lord Melbourne's office :—

October 26.

MY LORD—The conversation with which your Lordship honoured me this afternoon, including, as it did, your Lordship's opinion of the general character of the pensions given of late to scientific persons, induces me respectfully to decline the favour which, I believe, your Lordship intends for me; for I feel that I could not with satisfaction to myself accept at your Lordship's hands that which, though it has the form of approbation, is of the character which your Lordship so pithily applied to it.

The matter, however, was ultimately amicably settled, and Faraday enjoyed his well-merited pension to the end of his days.

In 1858, through the kind consideration of the Prince Consort, the Queen offered him a house on Hampton Court Green, in which he passed the rest of his life. He was now nearly seventy years of age. The dreaded reaction of that intense mental strain to which his mind had been subjected had long since set in; but thanks to the watchful care and tender solicitude of his wife, the evil consequences were but very gradual in their growth. In 1841, when he was

scarcely fifty years of age, he was obliged, through loss of memory and giddiness, to discontinue his researches for a time. He then rested almost entirely for a year, spending much of his time in Switzerland; and during the four succeeding years no further experiments in electricity were made, with the exception of an inquiry into the mode of working of Sir W. Armstrong's hydro-electric machine.

His mind was now gradually breaking up, and the knowledge of his increasing infirmities compelled him to resign, one by one, his various appointments. He now seldom left Hampton Court. A friend from London asked how he was: "Just waiting," was the reply. At another time he wrote: "I bow before Him who is Lord of all, and hope to be kept waiting patiently for His time and mode of releasing me according to His divine Word, and the great and precious promises whereby His people are made partakers of the divine nature."

On August 25, 1867, he passed quietly and peacefully away, dying, with scarcely a premonitory symptom, in his chair in his study. His funeral was strictly private; and, in accordance with his wishes, a gravestone "of the most ordinary kind" in Highgate Cemetery marks the last resting-place of one of the greatest and truest of experimental philosophers, and of one of the humblest and most tender-hearted of men.

In the foregoing sketch of the life and labours of Faraday we have had occasion more than once to offer our opinion as to the manner in which his biographer, Dr. Bence Jones, has accomplished his task. The autobiographical details of the work have been arranged with great tact and discrimination, and the desire to set forth the singular beauty and purity of his friend's

character is evident on every page. It is impossible to over-estimate the amount of good which such a book may do. On reading it one feels the more constrained to admit that "a mind fraught with integrity is the noblest possession."

IX

THOMAS GRAHAM

A LECTURE (WITH ADDITIONS), DELIVERED IN THE YORKSHIRE COLLEGE,
LEEDS, INTRODUCTORY TO THE EVENING CLASS SESSION, 1877-78.

THOMAS GRAHAM, one of the most original chemical thinkers of the nineteenth century, was born in Glasgow on 21st December 1805. The house in St. Andrew's Square, in the east end of the city, in which he first saw the light, is still standing. The Grahams belonged to Perthshire, but the father of the chemist, James Graham, had removed to Glasgow when very young, and ultimately became what was then styled a manufacturer. His business was sufficiently prosperous to enable him to give his son the benefit of a sound education. When scarcely six years old young Graham was sent to an English preparatory school, whence he was removed in 1814 to the High School, and at fourteen years of age he began his university career, entering, amongst others, the classes of Thomas Thomson on Chemistry and Meikleham on Natural Philosophy. He was a quiet, studious boy, conscientious and diligent in his work, but not otherwise remarkable among his fellows.

Thomas Thomson seems to have possessed, in a high degree, the faculty of communicating his own spirit of inquiry to his pupils. "Don't you think, Doctor," said

Graham on one occasion to his teacher, "that when liquids absorb gases the gases themselves become liquids?" This remark, coming from so young a pupil, and uttered at a time when the mutual relations of the physical states of matter were more vaguely understood than now, naturally impressed Thomson. The interest thus awakened in the young enthusiast never subsequently slumbered, and it was remarked by many that, in their meetings as members of the Philosophical Society of Glasgow, Thomson invariably treated Graham with an amount of respect and even deference which that brusque and quick-tempered old philosopher too frequently failed to extend to others.

Thomson's teaching gave a fixity to the purpose of Graham's after-life, and a desire to unravel some of those secrets of nature which Thomson's lectures had set him pondering upon took complete possession of his mind. His father had intended that Graham should enter the ministry, but his dislike to a calling for which he felt that he was not naturally fitted strengthened even to repugnance as he learned by a somewhat painful experience how inflexible was the determination of his parent. But Graham's mother sympathised with, even if she could not share in, the thoughts and aspirations of her son; and mainly by her self-sacrifice he was enabled to continue his studies for about two years in Edinburgh. He was in the habit of writing at great length to her, and his letters show the depth of his gratitude and affection. Whilst in Edinburgh he earned his first fee—some five pounds, by literary work—the whole of which he expended in presents to his benefactress.

In Edinburgh he attended the lectures and enjoyed the friendship of Hope and Leslie, and doubtless the

teaching of these distinguished men in no small degree contributed to direct his attention to that particular domain in science—the great borderland between physics and chemistry—in which his greatest triumphs were won. Returning to Glasgow, and acting on the advice of Meikleham, he sought to give lessons in mathematics ; but he quickly threw up this precarious means of livelihood, and established a private laboratory of the most modest description in Portland Street. In 1829 he was appointed Lecturer on Chemistry at the Mechanics' Institution in place of Dr. Clark, who afterwards occupied the chemical chair at Aberdeen ; but in the following year the Lectureship in Chemistry in Anderson's College fell vacant, and Graham was elected in succession to Dr. Ure, of dictionary fame. This event was the turning-point in his career. Although there was no endowment attached to the chair, and the laboratory was but scantily furnished, he had what he so long coveted—the means and the opportunity to carry out the promptings of his genius for investigation. It would be impossible to over-estimate the influence on science of the seven years which Graham spent at Anderson's College. During that time were sown all the seeds of the rich harvest of his after-years. Whilst in Glasgow he was elected into the Royal Society of London, in whose *Transactions* he had published his memorable "Researches on the Arsenates, Phosphates, and Modifications of Phosphoric Acid." In 1837 he removed to London, as the successor of Edward Turner in the recently founded University of London, now called University College. His election was in no small degree due to the good offices of Lord Brougham, whose vote was largely determined by the encomiums which Humboldt had passed upon the young philosopher. In

